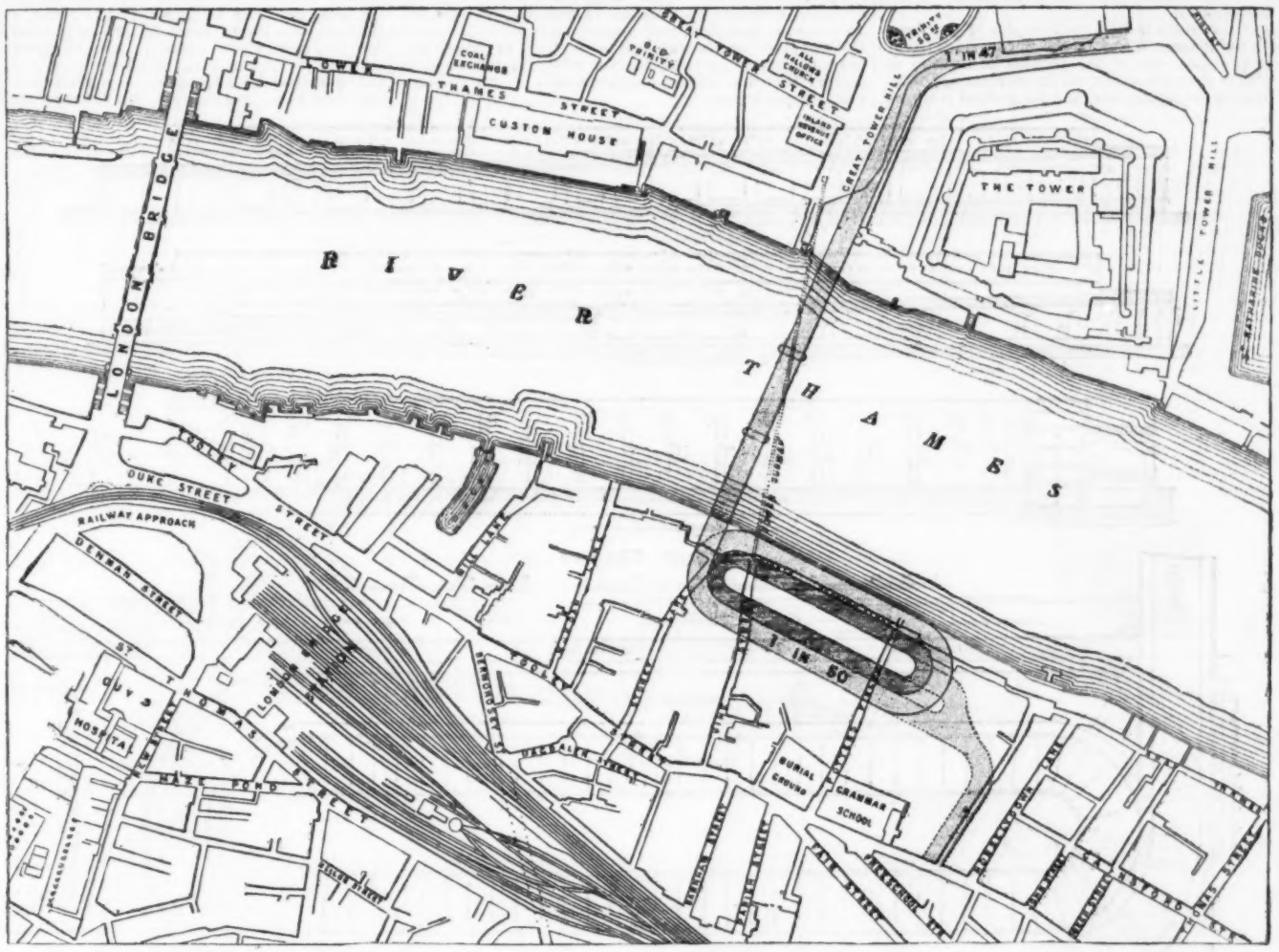
SCIENTIFIC AMERICAN

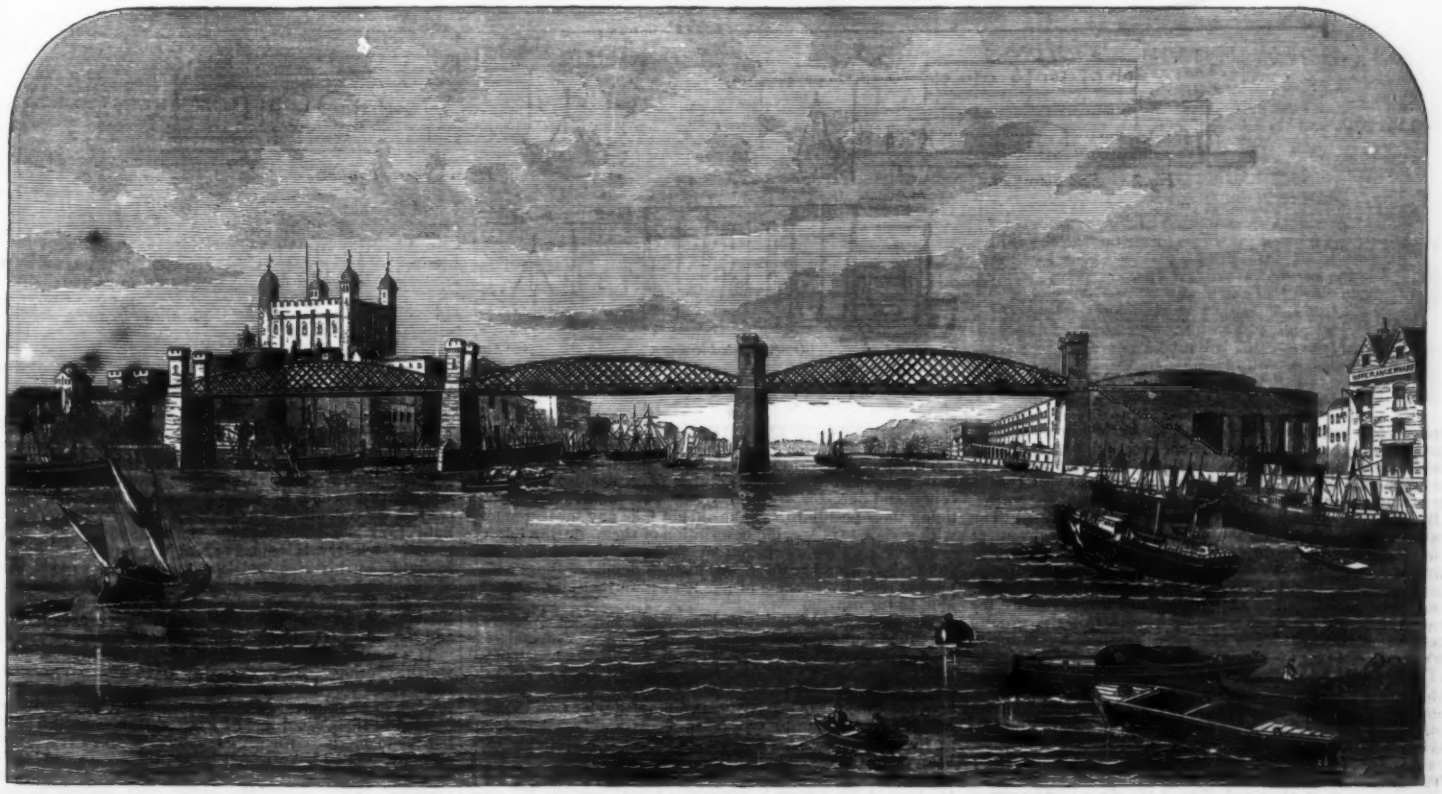
No. SUPPLEMENT 86

Scientific American Supplement, Vol. IV, No. 86.
Scientific American, established 1845.

NEW YORK, AUGUST 25, 1877.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.





PROPOSED NEW BRIDGE OVER THE THAMES, LONDON. HENRY VIGNOLES, ENGINEER.

PROPOSED NEW BRIDGE OVER THE THAMES, LONDON.

It appears to be generally admitted that something must be done by means of a new bridge or subway near the Tower, to relieve the traffic now daily encumbering the approaches to London Bridge, and the subject is now under consideration by a committee of the corporation, before whom various widely differing schemes, of more or less merit, have been proposed.

The latest proposal, by Mr. Henry Vignoles, is the erection of what may be called a mean-level bridge, of three spans of 300 ft. each, formed of lattice girders, and having a width of 60 ft., and a clear headway of 86 ft. above low-water level in the centre of the river, and 76 ft. at the abutments of the bridge.

This headway would suffice for the free passage of the ocean-going steamers still plying to London Bridge, while sailing vessels (the number of which is daily decreasing) could, with very rare exceptions, pass under the new bridge with top-masts housed.

The line of the bridge is from the west side of the Tower esplanade to the Phœnix Wharf on the Surrey side. As regards the approaches, one of the peculiar features of the proposed plan is the method adopted for overcoming the difficulty of that on the Surrey side, and this method is equally applicable to the approach for a high or low level bridge. This is to be accomplished by the erection of a gigantic warehouse of a special design, as shown in Figs. 3 to 9, along the sides and ends of which the approach road will rise to the bridge level by the easy gradient of 1 in 50.

By this arrangement several advantages are obtained. The lower end of the approach is brought to the most convenient point for access to the bridge, viz., Tooley street, instead of being at a much greater distance from the river-side traffic. The warehouse would be placed along the river-side to the east of the Surrey end of the bridge, and would replace the present inferior class of warehouses, most of the ground floors of which are below the level reached by ordinary floods.

There would be a direct access from its various floors to the new approach, which would pass round the building, while the whole of the river front would be available for wharfage accommodation, and almost the whole of the area occupied by the approach would be utilized as a warehouse space. The erection of this warehouse would, it is believed, recoup to a great extent the cost of land.

The approach on the north side of the river commences at the summit of Little Tower Hill, and skirting the Tower gardens and moat rises with a gradient of 1 in 47 on arches of a castellated design, till it reaches the north abutment of the proposed bridge at the bottom of Great Tower Hill on the west side of the Tower, without interfering with any of the existing Tower buildings.

Although the bridge is proposed to cross the river on the west or upper side of the Tower (see plan), yet owing to the approach on the Middlesex side commencing, as already described, at the east of the Tower, and that on the Surrey side at a point opposite the east side of the Tower, the convenience for traffic is practically the same as if the bridge and approaches were placed on a line between these two points, while the Tower wharf and the new wharf and warehouse would be still below bridge.

The saving of distance between the two starting points of the approaches on either side of the river, by way of the new bridge, as compared with the route over London Bridge, would be about 1,000 yards, or nearly three fifths of a mile; but owing to the narrow and crowded state of the thoroughfares on that route the saving in time might probably be counted by hours.

The estimated cost of the bridge, warehouse, and approaches, exclusive of purchase of property, is $600,000. This sum, at first sight, may appear large, but it is argued that any other scheme would be more costly as regards purchase of property, or compensation, while the revenue to be derived from the large warehouse would be very considerable.—*Engineering.*

Fig. 3. South Elevation.

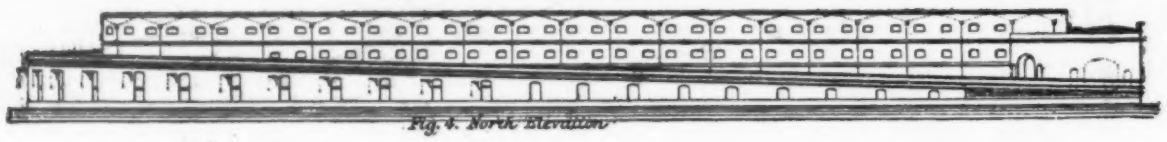


Fig. 4. North Elevation.

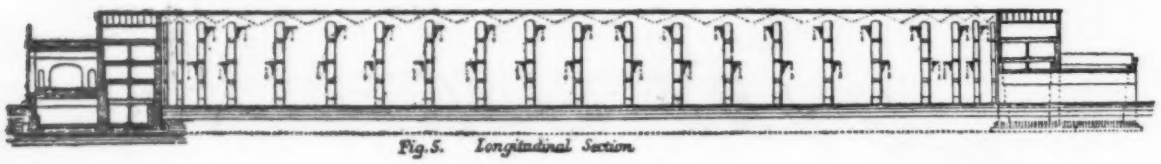


Fig. 5. Longitudinal Section.

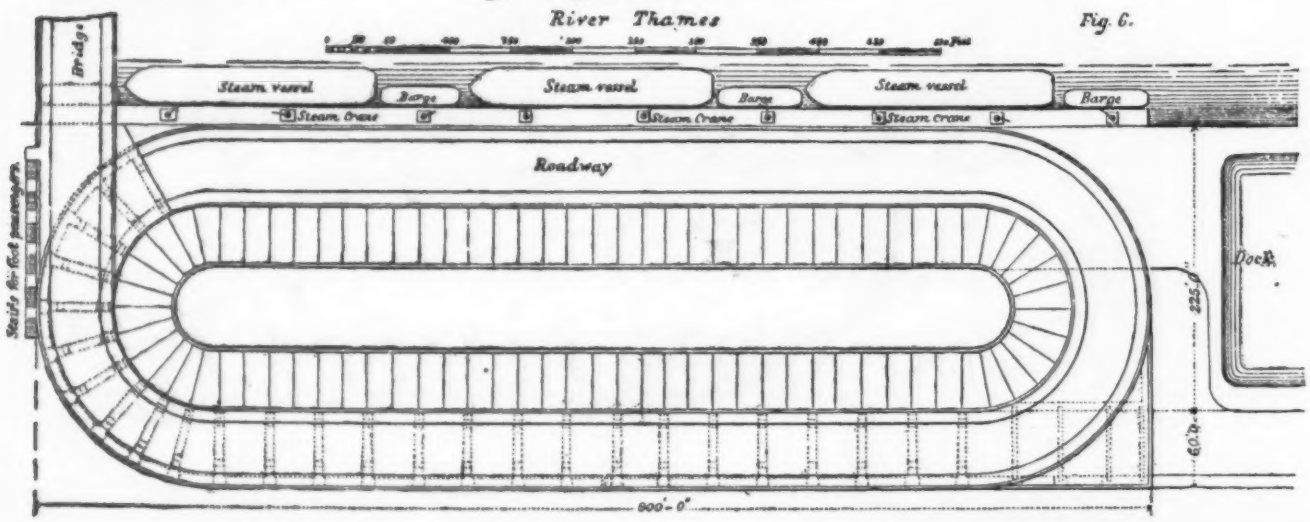


Fig. 6.

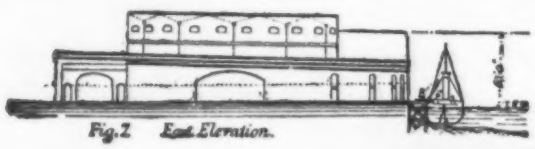


Fig. 7. East Elevation.

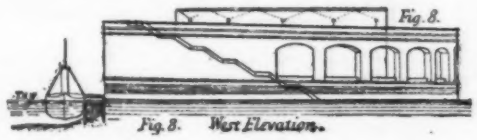


Fig. 8. West Elevation.

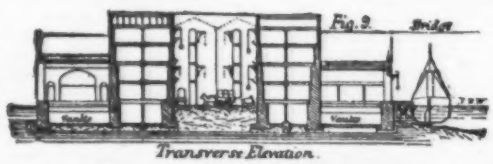


Fig. 9. Transverse Elevation.

PROPOSED NEW BRIDGE OVER THE THAMES, LONDON.

OBJECTS OF INTEREST IN PITTSBURGH.

There are a great many bridges in and around Pittsburgh, both road and railway structures, of a decided variety of types, and one can spend a day or two to advantage in noticing their peculiarities. We did not take any measurements with a view to a specific description of them, during our recent visit to Pittsburgh, but we presume that many of our subscribers would be interested in such details of those which possess novelty as some of our readers in that city might be able to furnish.

We spoke of the Point Bridge in our last issue. Across the Monongahela River is a suspension bridge of several very moderate spans, built by Roberts, and evidently constructed many years ago, in the comparatively early days of suspension bridges and wire rope. On the other side of the city, crossing the Alleghany River, is a more modern suspension bridge by Roebling, of two main spans and two half spans at the ends. The towers are of wrought iron, and the bridge is stiffened by a straight truss so as to be very quiet under the traffic. Inclined suspending ropes, running from the tops of the towers to numerous points between the piers and the quarter span, are intended, as usual, to assist the main cable.

The second bridge above this one, on the same river, is a wrought iron, riveted lattice of five spans, with shorter spans of heavy plate girder at each end. It is a double track bridge with three trusses, and on one span the trusses diverge so as to accommodate a switch and three tracks at the shore end. It belongs to the Pittsburgh, Fort Wayne & Chicago R. R. This structure does not strike one very favorably, as the parts, both chords and lattice bars, appear to be of the same sections at all points of the span, not an economical disposition of material.

A little distance above on the same river may be seen one of the old fashioned Burr Truss highway bridges of four spans, built entirely of wood, even to the tension members, each truss being assisted by a wooden arch. There are also, as we have said, several other bridges, of wood and iron, which possess no noticeable features.

Upon Forbes Street, however, may be found, at a distance of perhaps three miles from the center of the city, a very pleasing and airy looking structure, which carries in a single span this street over a deep ravine. It is a light circular arch, of concentric ribs with riveted diagonal bracing, and was designed by Mr. Pfeiffer, who, after his experience with the St. Louis Bridge, undoubtedly turned off this structure, which resembles it in miniature as to type, very easily. It was, if our recollection serves us, illustrated and described in *Engineering* soon after its erection, and this description was copied by the *Scientific American*. As we are penning these few remarks away from our office, we have not access to our files and cannot speak more definitely.

About a mile from this bridge, across the same ravine, where it widens out on nearing the Monongahela, has been erected the Sylvan Avenue Viaduct, which consists of several light iron skeleton piers of considerable height suitably braced at intervals, connected at the top by trusses which resemble an inverted bow-string girder, the panel lengths of the tension member being straight. As the truss has considerable depth for its span, the bottom chord has therefore a very decided angularity, if we may so call it, and the effect, as viewed from below, is far from agreeable.

The works of the Keystone Bridge Company are well worth visiting. This company have just completed the large truss for the Cincinnati Southern Railway, now in process of erection over the Ohio, and are not very busy at present in their usual line of work, but are engaged in building some engines for an extension of the Lucy Furnace, close by. They have excellent facilities, however, for prosecuting bridge construction, possessing large and well-arranged shops, assembling-house, and draughting-rooms. We inspected with much interest several special machines, many of them of their own design, which they have added to the usual plant of such works. Among these may be

mentioned the rolls for finishing their patent rivetless column. This column, it will be remembered, is composed of segments with outside flanges, akin to the well-known Phœnix column, but fastened by a trough-shaped strip of iron placed over the flange and compressed by the rolls above mentioned so as to make a somewhat dovetailed joint, holding the segments firmly together.

The company have also, after long experimenting, succeeded in constructing an automatic punch which will punch the required holes in strips of plate, angle, or other iron, with whatever spacing is desired, and with great accuracy, as an attendant revolves a hand-wheel with accompanying index. Each complete revolution of this wheel moves the bar, or piece to be punched, one inch. At one side of the shop may also be seen the special machine for facing the ends of the St. Louis bridge arch sections, so as to secure the exact angle of obliquity of the end to the axis of the section, and the arrangement for making the coupling grooves. They have also a milling machine for bringing eye-bar heads to any desired shape, the cutters being guided by a template.

Their large testing machine, working by hydraulic pressure, will take in columns or bars, for compression or tension, up to thirty-five feet in length, and is capable of exerting a force of five hundred tons. We understand that quite extended experiments have been made upon various columns and forms of strut, and we hope that the results may soon be published. Their upsetting machine for heavy work also acts by hydraulic pressure, but they have adapted a machine, originally intended for mining coal by repeated blows, to the upsetting of light bars. It appeared like a rude contrivance, but certainly did its work rapidly and well. The riveting machine has a movable post, balanced or kept up to its place by a weighted lever. In case a rivet is too long, or if, for some reason, there is unusual resistance, the post will yield, turning about its foot, in place of breaking. A patent clutch, also, is used on this machine, as well as on the punches, which ensures that the punch shall stop at the end of the back stroke.

At the Crescent Steel Works, which are near to the shops of the Keystone Bridge Company, may be seen the manufacture of crucible steel and its conversion into sheet, spring and bar steel, under the rolls or steam hammer. They use here the Siemens Regenerative Furnace, making the gas for all the works in one place, and sending forth from the top of the chimney stack but little more smoke than is seen issuing from the chimney of an ordinary dwelling. The iron pipe to the chimney was scarcely too hot for the hand, and here was afforded a beautiful example of the scientific and economical combustion of fuel. If all the iron furnaces with which both rivers are lined should introduce similar improvements, the change would make a decided difference in the quantity of inky smoke which is now wastefully vomited forth upon the city and surrounding country. The fuel is the very small bituminous coal or slack, formerly considered refuse, and with these furnaces it takes but sixteen bushels of slack to melt one ton of steel. One man can handle all the coal and ashes for large works. It is at these shops that they grade their ingots by ocular inspection. The accuracy of this method of determining the per cent of carbon, by the appearance of a fresh fracture of an end or corner of an ingot, has been demonstrated by repeated chemical analyses, and other physical tests have proved that the desired mechanical qualities of the ingot depend upon the amount of carbon. A difference of five one-hundredths of one per cent of carbon can be detected by the eye, and the expert marks upon the ingots their different grades as fast as he can handle the chalk. We saw the ingots marked and could see for ourselves distinctive differences in the lustre of the fractured surfaces. Some five or six grades were picked out by the marker by a glance of the eye. There is, therefore, needed only this discrimination as to quality to enable the manufacturer to supply the engineer with the grade of steel which he desires and definitely specifies.

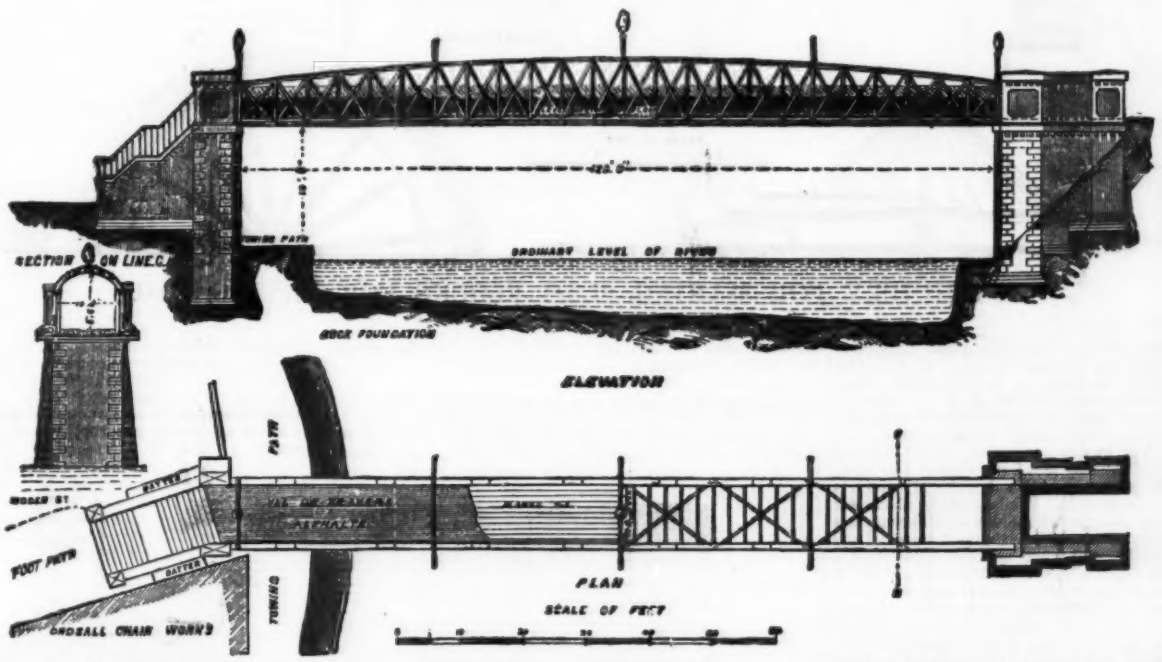
A proof of these statements will be found in the article contributed to our columns by Prof. Langley and published in the current volume recently.—*Engineering News.*

OPEN CONTRACT FOR FOOT-BRIDGE, MANCHESTER.

THIS bridge is intended to form a means of communication for foot-passengers over the river Irwell, from Woden street, Salford, to Hulme Hall-road, Manchester. The abutment and steps on the Salford side have already been built up to the level of the top of the bed stones, which are also fixed, but will have to be put in a thorough state of repair by the contractor. About one square yard of 9 inch brickwork requires rebuilding, 6 feet of ramped coping must be refixed, all the existing work must be pointed out in blue lias lime, and the whole of the coping to be drilled and dressed off to receive the iron railing, in order to complete the abutments and steps up to the above-mentioned level. Above that level the pilasters are to be carried up, and the whole to be finished as shown on the drawings and in accordance with this specification. The whole of the abutments and wing walls, including the excavation and foundations on the Manchester side of the river, is included in this contract. The abutments shall be constructed of the best hard burned common red bricks, sound, square, of uniform size, and otherwise of good and approved quality, set in mortar made of blue lias lime as hereinafter specified. The outside or face work where exposed to view shall be of the best pressed bricks that the neighborhood can produce, and shall be carried up in regular courses and properly loaded with the common brick backing. The bricks shall be soaked in water before being laid, and every brick shall be rubbed up and every joint flushed in with mortar, so as to form a solid joint all round. All soft, inferior, or broken bricks will be rejected. The whole of the brickwork shall be brought up in regular courses at a uniform height throughout in old English bond; the horizontal joints being of such a thickness that four courses laid in mortar shall not be more than 1 inch higher than four courses laid dry. All joints where exposed to view to be ruled and very neatly finished off in a workmanlike manner.

Insert in the abutment, 6 inches above the ordinary water level, two 3 inch tile drains set in cement 6 feet apart, two 3 feet 6 inches apart, and two more at a height of 6 feet 6 inches above the ordinary water level.

No trial holes have been made to ascertain the nature of the strata; parties must, therefore, form their own judgment of the quantity and value of the work and the nature of the ground to be excavated, as the contractor will be required to provide all necessary cofferdams, machinery, or whatsoever may be necessary to keep the ground clear of water, free from extra charge, as no allowance will be made for any additional plant or materials to carry on the work.



WODEN STREET FOOT BRIDGE, SALFORD, MANCHESTER. A. M. FOWLER, M.I.C.E., ENGINEER.

The contractor will be required to excavate to the full extent shown on the drawings, or to any additional depth or width which may be required for working room. Any additions or deductions from the works shown on drawings to be paid for according to the schedule of prices attached to the tender, without reference to the depth of the work below the surface of the ground. No work shall be commenced in any excavation until the engineer shall have inspected the same. The materials arising from such excavation shall be deposited so as to form part of the embanked approaches to the bridge. The contractor to find all necessary scaffolding, piling, cofferdams, and tools, and machinery of every description necessary for the perfect execution of the works.

The surplus material, if any, to be disposed of by the contractor at his entire risk, and the works to be left free of all surplus material thoroughly cleansed at the completion of the work.

The material for making concrete to consist of stone, gravel, and sand mixed in proportion of one part of stone broken to pass through a 2-inch ring, one part of clean, coarse, sharp gravel free from earthy matter, and one part of clean, coarse, sharp sand. Five parts of this material to be mixed with one part of fresh burned blue lias lime. The proportions to be accurately gauged and mixed in a dry state, with proper wooden measures on a wooden platform. It is then to be moistened with just sufficient water to make it set properly, and well beaten until a thorough admixture of the parts is effected, and immediately tipped from a height of not less than 10 feet into the place where it is required, so as to need as little levelling as possible, the shoots being shifted for that purpose. It is to be brought up in layers not exceeding 12 inches thick, each layer to be completed and brought up to a level surface before any other layer is commenced. The lime is to be blue lias lime, fresh colored, freely ground, and carefully preserved from moisture until actually used. No brickwork must be built upon the concrete until it has thoroughly set, and its finished surface has been examined by the engineer.

All mortar used to consist of fresh burned lias lime, except for the pointing above the ground line, where Ardwick lime is to be used, and clean, coarse, sharp sand, in proportion of two measures of sand to one of unslacked lime, the lime to be ground under rollers, then mixed with the sand and just sufficient water to make it set properly, and thoroughly tempered in a mortar mill, and made daily as required for use. Mortar which has previously set or become hard shall not be permitted to be used in any part of the work.

The cement must be of the best quality of Portland cement, by an approved maker, recently made, and must be mixed up with clean, sharp river sand in equal proportions; no cement to be mixed up except it is to be wanted for use, and that which is mixed must be used immediately, before it has set or stiffened in the least, or it will not be allowed to go into the work, otherwise all work so constructed will be rejected; a duplicate invoice of each cask to be delivered to the engineer.

The stone for quoins, bed stones, copings, and other work, except steps, shall be from the hardest and best beds of quarries at Fletcher Bank, Ramsbottom, hard, sound, of uniform color and texture, and free from spots, flaws, and other imperfections whatever; all the stone shall be tooled, well boasted on the beds and joints, and laid on its natural bed. No chipped or broken stones will be allowed. No stone to be less than three feet in length. The bed stones for the girders to be in one block and sunk to fit the ends of the girders, and the caps to pilasters to be in one stone. The stones in the impost course to be put together with slate dowels. The coping stones to stair walls to be jointed with 1½ inches diamond-shaped vertical cement dowels the full depth of the stone. All coping where shown to parapets to have throating cut throughout.

The existing flight of steps from Woden street to the towing path and retaining wall to be pulled down and rebuilt wherever defective. Three new steps will be required to replace broken ones, to be of the best stone from the Haslingden quarries, and similar in all respects to the existing ones. The present coping must be reset and holes sunk at proper distances to receive the iron railing.

Iron railing must be provided and fixed on the coping of the retaining wall, both sides of the steps of the Salford abutment, and also the flight of steps to towing path. The railings to consist of vertical bars ¾ inch square, 6 inches apart and 4 feet 6 inches high, connected together at the top by a horizontal half round rail, 2½ inches wide and ¼ inch thick, and run with lead 2 inches into the aforesaid coping of steps and wall. The iron to be of the best Staffordshire bar iron, and have one coat of good red lead paint and two coats of an approved color.—*The Engineer.*

WINGS *vs.* WHEELS.

THERE was lately a race from Dover to London between the continental mail express train and a carrier pigeon. The pigeon, which was bred by Messrs. Hartly & Sons, of Woolwich, and "homed" when a few weeks old to a building in Cannon street, City, was of the best breed of homing pigeons known as "Belgian voyageurs." The bird was tossed through the railway carriage window by a French official as the train moved from the Admiralty Pier, the wind being west and the atmosphere hazy, but with the sun shining. For upwards of a minute the carrier pigeon circled round to an altitude of about half a mile, and then sailed straight away towards London.

By this time the train, which carried the European mails, and was timed not to stop between Dover and Cannon street, had got up to full speed, and was proceeding at the rate of 60 miles an hour towards London. The odds at starting seemed against the bird, and the railway officials predicted that the little messenger would be beaten in the race. The pigeon, however, as soon as it ascertained its bearings, took the nearest homeward route, the distance "as the crow flies" between Dover and London being 70 miles, and by rail 76¾ miles. When the continental mail express came into Cannon street station the bird had been home 20 minutes, having beaten her Majesty's Royal mail by a time allowance representing 18 miles.

MACHINERY FOR MAKING SLAG BLOCKS.

THE question of utilizing furnace slag is one that has exercised members of the iron trade probably for as long as smelting has been practiced, but, until lately, without any very tangible result having been arrived at; and at present the scoria or refuse produced in the operation of smelting iron ore is nothing more than so much useless matter, the removal of which forms a large item of expenditure with iron manufactures. It is highly probable that not more than a small part of all the slag produced in the operation

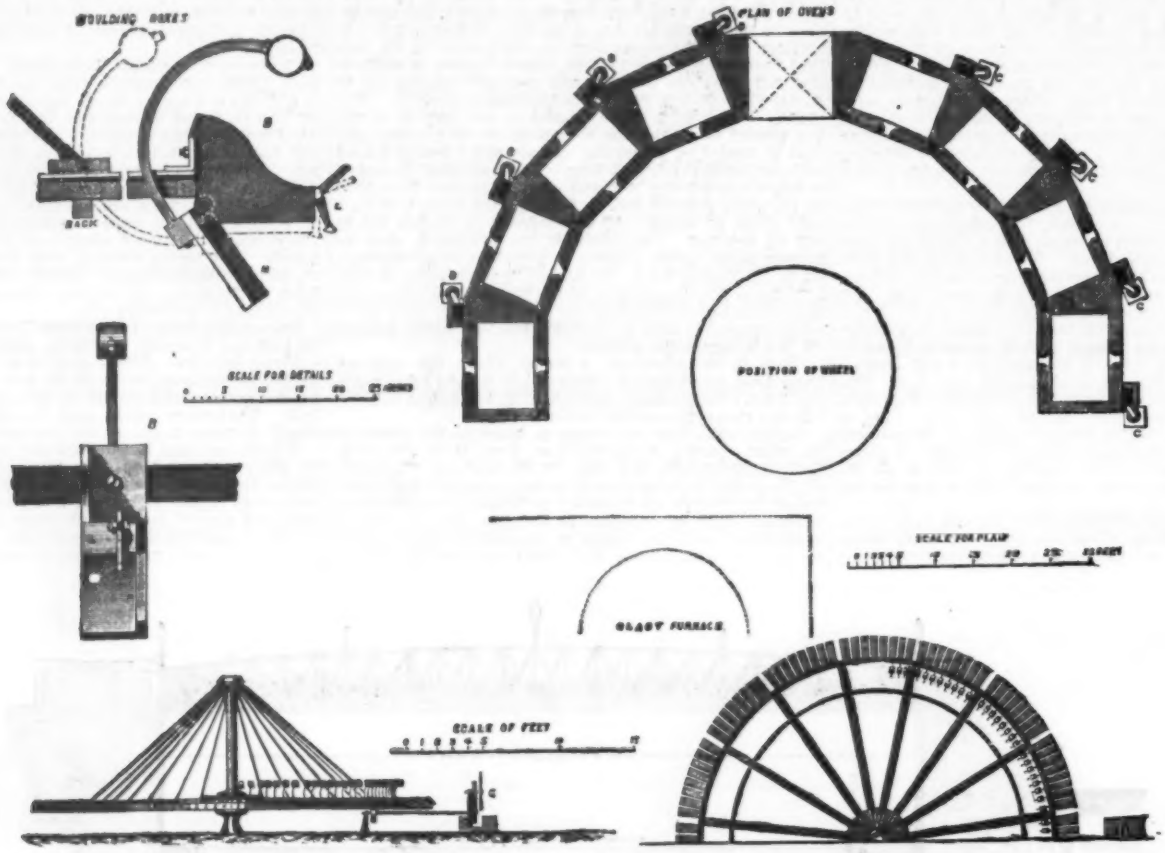
of smelting will ever be turned to account by manufacture into a marketable commodity.

The process for converting slag into materials for paving streets and footpaths and other purposes is already in operation. Works have been erected and are being carried on at Acklam and Lackenby, near Middlesbrough, and also at Tipton, in Staffordshire, with considerable success. The patentee of the process is Mr. Joseph Woodward, of Leamington, who has devoted considerable time to perfecting his process, which, as now carried on, is one of great simplicity. The apparatus for moulding the slag consists of a horizontal wheel of 20 feet or 25 feet in diameter, which revolves upon a vertical axis. It is constructed almost wholly of ordinary T iron, and is braced with rods so as to render it perfectly rigid. Round the margin of the wheel are attached a number of cast iron boxes or moulds, as shown in the detailed cut, for the reception of the slag as it flows from the furnace. These boxes are made with a movable bottom M, which is secured in its place by a latch L. Whilst making blocks for general stock it is usual to employ moulds of different sizes and shapes at the same time, leaving the assortment of the blocks until they are cold; but when any particular kind is required urgently, the entire wheel is fitted with moulds all of the same pattern, which can be done quickly and with ease.

The whole apparatus is made to revolve by means of hand wheels and gearing, shown at G, and so the boxes are filled one after the other as fast as the slag flows from the furnace. As soon as the slag flows into the mould it begins to cool and its bright red color to fade, but it does not become sufficiently solid to be moved until the wheel has made about a quarter of a revolution. At this point a boy tips the latch up, and the bottom of the box falls down, as shown in the engraving, allowing the box to slide out on the ground. When released from the mould the brick or paving block is consolidated only on the outside, the greater part of it being still in a molten condition. As the brick falls it is lifted up by a boy with a three-pronged fork, and delivered to a man who transfers it to one of the annealing ovens, which, as a matter of convenience, are arranged concentrically with the wheel. On being placed in the oven, the angles and corners of the brick, which had lost their color and faded, again become red and the whole is maintained at a bright red heat for twenty-four hours, after which the dampers are closed and the whole kiln is left to cool, which it does sufficiently to admit of the contents being removed in about twenty-four hours more. The designing of a box suitable for moulding the slag was a matter of some difficulty, as it was necessary that the mould should be perfectly square at the angles, and it was found in the first patterns that the blocks, when made, could not be conveniently removed from the moulds, as the amount of shrinking was not sufficient to separate the block from the sides of the iron box. The box now used, however, answers its purpose effectually. A weight is fixed in connection with the movable bottom of the mould, and so adjusted that the bottom remains closed so long as the box is empty, but that it falls down by the weight of the slag block when the latch is released.

The ovens employed for annealing the blocks are simple in construction, as will be seen from the plan, and do not require a large quantity of coal to produce the necessary heat. It is in most cases, moreover, practicable to utilize the waste gas from the blast furnaces for bringing up the temperature of the ovens; and when this can be done, a very large proportion of coal may be saved. The waste gas is conducted round the back of the ovens by an underground pipe, which is connected with the several blowers shown at C, and can be directed, by stopping certain valves, to any particular oven.

The bricks and paving blocks turned out by the process are very hard and compact; and judging from their power of wear under traffic, as illustrated at Stockton-on-Tees, Darlington, and elsewhere, they evidently possess in addition the property of toughness, so essential in a material intended to resist heavy traffic. The weight of the manufactured slag is 168 lb. to the cubic foot, and the blocks are very neat and square, affording under traffic a fine level surface, productive of little noise, and apparently not prone to become slippery. The manufactured slag can be applied to various purposes besides the paving of streets. Tram road blocks, sewer bricks, and a variety of other articles can be produced, which, from their accuracy of form, comparative cheapness, and impermeability to moisture, are likely to be favorably received. The crushing strain of the manufactured material is something over 8,000 lbs. to the square inch.—*The Engineer.*



MACHINERY FOR MAKING SLAG BLOCKS FOR PAVING, ETC.

REGAN'S SMOKELESS FURNACE GRATES.

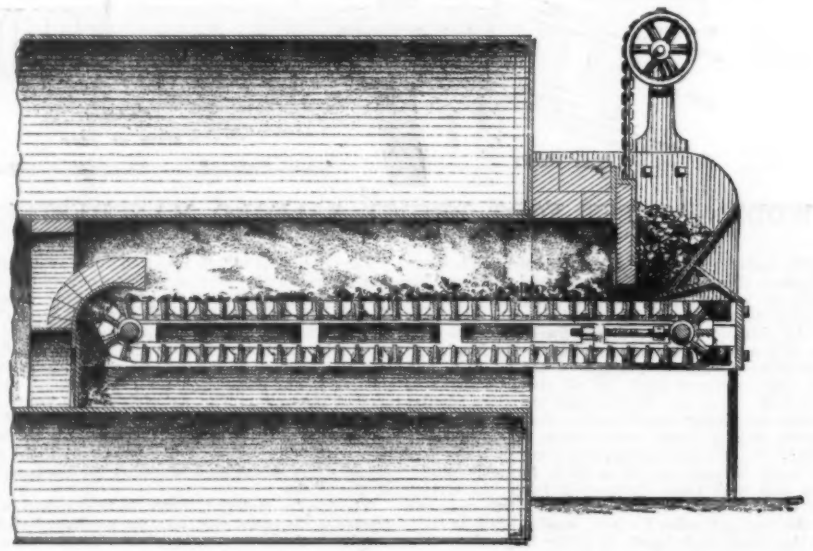
In this improvement the grate is made in the form of an endless belt, which is moved by screw gears. The fuel is fed from a hopper, as shown. Practical trials in England give, it is said, the following results:

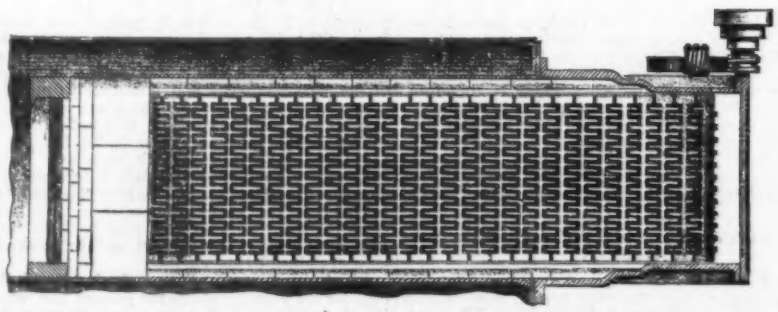
Economy in coal consumption, as they are specially adapted for the use of *small* coal; absolute prevention of smoke; the apparatus is simple, durable, and easily applied; the combustion of the coal is complete.

Dimensions of Boiler.—33 feet long × 7 feet diameter; single flue, 3 feet 2 inches, having 11 Galloway tubes; fire grate, 6 feet × 3 feet 2 inches; area of grate, 19 square feet.

Coal consumed in one hour	398 lbs.
Water evaporated in one hour	52·1 cubic feet.
" " per 100 lb. of coal	17·5 "
" " per 1 lb. of coal	10·5 lbs.
Coal consumed per square foot of grate	15·8 "

Cost of small coal per ton, 10s., as used in the machine furnace.





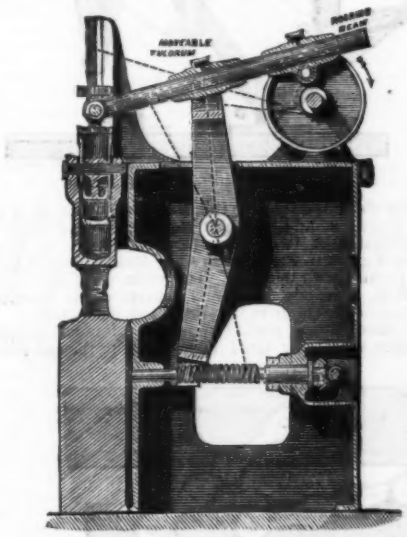
REGAN'S SOFT COAL SMOKELESS FURNACE GRATE.

THE OTHEOSCOPE.

So MANY different forms have been given to the radiometer by the ingenuity of Mr. Crookes, that at the Royal Society's *soirée* in April he was able to exhibit as many as twenty modifications of the little instrument. Among these modifications were several so different from the earlier forms of radiometer, that Mr. Crooks proposes to distinguish them as *Otheoscopes*, a name derived from ὠθέω, I propel. In an ordinary radiometer, whatever form it may assume, the glass globe is an essential part of the apparatus. Without the globe the fly would never turn, and the most probable view of the radiometer—the view to which the inventor himself assents—is that rotation must be caused by the excess of molecular pressure between the fly and the enclosing vessels. In the otheoscope, on the contrary, the glass bulb serves merely as an envelope to enclose the rarefied air, and is by no means an essential part of the machinery. In fact, an otheoscope has been constructed without any envelope whatever. Let a radiometer and an otheoscope be removed to a position in space where the air is extremely attenuated, so that the atmospheric pressure is perhaps not more than a millimeter of mercury, and then remove the glass case from each instrument; the fly of the radiometer immediately would become stationary, though there might be abundant solar radiation, but the fly of the otheoscope would whirl round as merrily as before. The first form of otheoscope described by Mr. Crookes ("Chemical News," May 4, 1877), consists of a four-armed fly, each carrying a vane of thin clear mica. At one side of the glass bulb which encloses the apparatus there is a vertical plate of mica blackened on one face, and so placed that each vane closely approaches it as the mill rotates. If light be allowed to fall only upon the clear vanes, no motion is produced; but if the light shine upon the black plate the vanes instantly begin to rotate, as though repelled by a molecular wind blowing from this surface. The movement is therefore produced by pressure generated on a fixed part of the apparatus, by which the movable portion is propelled. As this driving-surface is stationary, it is not restricted in weight, size, or shape; and hence the modifications of which the otheoscope admits are well-nigh endless.

IMPROVED FORGING HAMMERS AND STAMPS.

THE accompanying engraving represents a novel arrangement of forging hammers and combined forging hammers and stamps, now being manufactured by Messrs. J. Scott Rawlings and Co., engineers, of Lionel Street, Birmingham, being the invention of Mr. Daniel Longworth, manager of the firm. The hammers and stamps are driven by belts, and are of the kind in which a cushion of air is interposed between the driving mechanism and the striking weight or hammer, thus relieving the mechanism of the shock caused by the blow, and allowing for variations in the thickness of the work under operation. In forming this elastic connection, the packing glands and loose cylinder covers hitherto generally used are entirely dispensed with. The main piston of the hammer forms a cylinder for an internal piston rigidly connected to the hammer by a pin passing through the latter and the internal piston; the upper parts of the cylinder being quite open, form chambers which can be readily filled with oil or other lubricant. One great defect in other hammers driven by belts, and in steam hammers controlled by the foot only, is that when a light blow is required the number of blows given per minute has to be reduced in proportion to the lightness of the blow, which is quite contrary to the requirements of forging or hammering. In these hammers light and heavy blows are delivered at the same rate, the force of the blow being regulated in a positive manner by giving the hammer or stamp a longer or shorter lift from the anvil block, thus diminishing or increasing its velocity. This variation of the force of the blow is effected in the following manner. The connection between the main piston of the hammer and the driving mechanism is a round bar forming a lever rocking on a movable fulcrum formed by

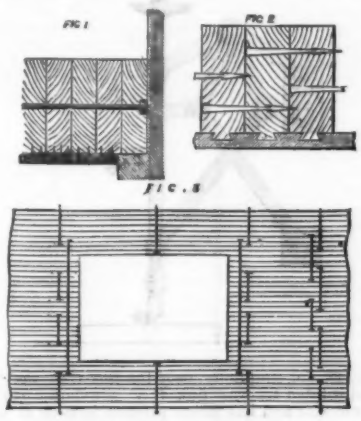


IMPROVED FORGING HAMMER AND STAMPS.

conical steel centres carried on a shifting lever, this fulcrum being either in the middle of the rocking lever as in the forging hammer, or at the end as in the combined hammer and stamp. The movable fulcrum is placed under the control of the workman by the forked lever which embraces the starting or foot-lever and the small belt which drives the countershafts and screw in connection with the shifting lever and movable fulcrum. By moving the forked lever to right or left with the foot, the fulcrum is moved in either direction, causing the blows to be rapidly varied as required; when the forked lever is kept in mid position the fulcrum remains stationary, and blows of uniform force are given. By means of a handle placed on the end of the countershaft, the force of the blows can be also varied by the hand of the attendant. When the workman takes his foot off the starting or foot-lever the hammer stops. Another novel feature in these hammers and stamps is the great acceleration of the velocity of the hammer in the down stroke, which causes a heavier blow to be given in these than other hammers of the same weight and rate of blows per minute. In the combined hammer and stamp, single blows can be given by merely pressing the starting lever with the foot and raising it again, the hammer or stamp always rising and remaining at the top of its stroke, one of the requirements of a perfect stamp. It will be seen from this description that these machines are suitable for all kinds of forging, hammering, and stamping, requiring, it is claimed, much less power for the same blow than the self-acting steam hammer, and they may be driven by water or other power where steam hammers are unsuitable.—*The Engineer.*

FIRE-PROOF CONSTRUCTION.

IN several well-known instances so-called fire-proof buildings in London have shown at least that the name has been misapplied. It has been assumed that construction with fire-proof materials is fire-proof construction, but the fallacy of this has been illustrated by several disasters, which have called forth comments of a very unfavorable character from the chief officer of the Fire Brigade. Messrs. Evans and Swain, of Southampton street, Strand, have obtained a patent for a system of constructing floors and roofs made up mainly of joists placed on edge and bolted or spiked close together. The solid floor thus made is plastered or cemented underneath, or, in the case of a roof, concreted or cemented on the top so as to form an air-tight structure, and to take advantage of the well-known difficulty of burning large masses of wood without a good current of air. We herewith give transverse sections of the floors thus formed, Fig. 1 being bolted and Fig. 2 spiked together, and both showing different methods of holding the plaster. A portion of a plan of a floor bolted together, and with an opening such as is necessary for many purposes, is given at Fig. 3. For a floor unweakened by any such hole as this, the comparative depths of the joists, as given below from the estimate of the patentees, would be sufficient for all ordinary purposes, but when a large hole is made for a lift, the greater part of any weight supported on that part of the floor made by the joists cut off at the hole is transmitted by the bolts to the adjacent



FIRE-PROOF CONSTRUCTION.

uncut joists, which in such a case would have to be much increased in depth—a contingency not allowed for in the following estimate, and which would make a much greater increase in the cost of the solid than in the ordinary open joisted floor. A few additional bolts will not, as suggested in the patentees' pamphlet, make up for the loss of strength at such holes; the additional strength must be in the joists into which the bolts are to pass. The following are the estimated relative costs of the two systems as published by the patentees:

Cost of a floor 20ft. by 15ft.—containing three squares—constructed in the usual manner.

42ft. × 4½in. × 3in. fir plate = 4ft. cube at 2s. 6d.	£0	10 0
44ft. × 11in. × 3in. fir framed joists } = 64ft. 4in. cube at } 2s. 9d.	8	16 11
5ft. × 11in. × 4in. ditto trimmers }		
20ft. × 15ft. 1in. sound boarding and fillets = 3 sqrs. at 20s.	3	0 0
20ft. × 15ft. 1¼in. flooring in narrow widths = 3 sqrs. at 50s.	7	10 0
20ft. × 15ft. lime and hair rugging = 33⅓ yds. at 6d.	0	16 8
20ft. × 15ft. lath, plaster, float, and set ceilings = 33⅓ yds. at 1s. 9d.	2	18 4
3	23	11 11
= per square	7	17 4

Constructed according to the patent.

21ft. × 16ft. × 7in. batten and spikes = 196ft. cube at 2s. 9d.	26	19 0
21ft. × 16ft. planing = 336ft. sup. ½d.	0	14 0
20ft. × 15ft. plastering, etc., to ceilings = 33⅓ yds. at 1s. 6d.	2	10 0
Royalty, 3 squares at 10s.	1	10 0
3	31	13 0
= per square	10	11 0

For such a floor, therefore, the cost upon the latter estimate is about 44 per cent. greater than the former, which, it must be admitted, would be an unusually good and strong floor, though not fireproof. The cost on Messrs. Evans and Swaine's method would, however, be slightly reduced by the saving of 5in. in the height of the walls, but in many instances this would be outweighed by the cost of leveling and planing the surface, or covering it with boards when a good floor surface was required.

In point of weight, the system offers some advantage over many previously proposed methods of construction, and would undoubtedly resist the heat which has destroyed many so-called fire-proof buildings. Builders should, however, be warned against thinning the walls on account of their adopting these floors—a suggestion made by the patentees without apparently any reason. Many ordinary floors, with ceilings underneath, might be made capable of resisting fire for considerable length of time by carefully fixing a layer of sheet iron between the joists and boards in such a way as to exclude all air currents.—*The Engineer.*

RAPID FILTER.—In the *Zeitschrift für Analytische Chemie* a simple contrivance for accelerating filtration is given. To produce a vacuum Herr C. Holthof uses an ordinary tin syringe, the lower tube of which is unscrewed and replaced by a doubly-bored gutta-percha plug through which two glass tubes are put. Upon one of these tubes a gutta-percha Bunsen's ventilator, opening outwards, is placed; the other tube is bent round and expands. The wider end is closed by a second cork, through which a third glass tube is passed, and in the interior of the expansion a second ventilator is attached to the third glass tube, while its outer end is connected with a large bottle. The cork of this last is bored in three places, one aperture serving to form the connection with the apparatus already described, one communicating with a manometer, and the third with the vessel containing the liquid to be filtered. By drawing out the handle of the syringe a vacuum, or, rather, a space containing rarefied air, is formed in the bottle, and is a material aid to rapidity of filtration.

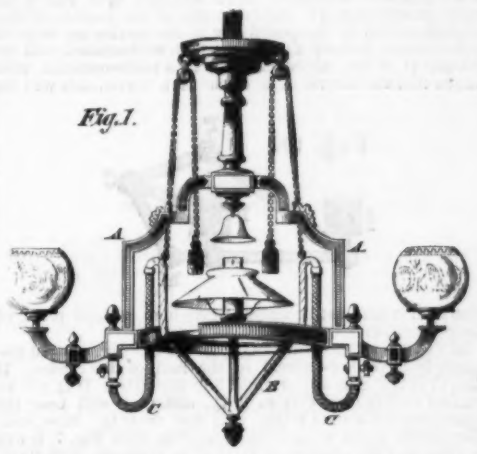
THE GILDING AND PLATING OF GLASS AND PORCELAIN.—Sulphur is dissolved in essence of lavender (*oleum lavandi, spicæ*) till the consistence is semi-fluid. With it is mixed a solution of chloride of gold or chloride of platinum, in ether, and the mixture is concentrated at a gentle heat. It is then applied with a brush to the surfaces to be metalized, after which the objects are carefully heated in a muffler till the sulphur and the volatile matters are eliminated. There is thus formed a coating of gold or of platinum, upon which a uniform layer of silver, gold, or platinum may be deposited by the galvanic process.—E. HANSEN *in Poly. Notiz-blatt.*

TAYLOR'S IMPROVED DROP LIGHT.

THIS improved drop light is the invention of W. B. S. Taylor; a perspective view is given in Fig. 1, and an enlarged outline view of the principal parts in Fig. 2. It consists in the employment of flexible tubing to conduct the gas from the stationary pipes in the frame work to the burner attached to the movable or drop portion.

By reference to the cuts, A A is the stationary frame work through the pipes of which the gas is received from any convenient source. Attached to orifices of the frame work are seen the flexible tubes C C which serve to conduct the gas to

Fig. 1.

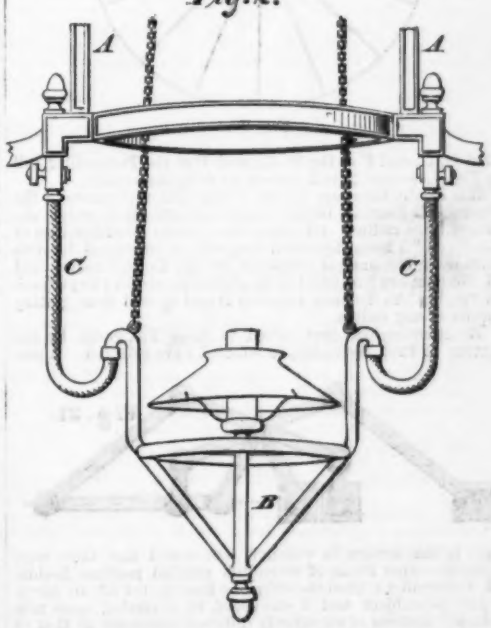


the burner attached to the pipes B. The drop is suspended to the fixed portion by chains which pass over pulleys, and is balanced as is usual in such appliances. The flexibility of the tubes enables the drop to be lowered at pleasure, and when raised to its position, they hang in natural and graceful curves on each side of the drop.

The advantages claimed for this improvement are that by the employment of the flexible tubes a great variety of elegant designs may be made, which will enable manufacturers who adopt the use of the tubing to present to the market a cheap yet well-made drop light or chandelier.

The tubing is expressly prepared for this purpose by the Union Tubing Co., of 365 and 369 Broadway, New York city, and is of extra strength and capable of resisting an internal pressure of fourteen pounds to the square inch.

Fig. 2.



In the manufacture of the light, it is necessary that the tubing be below or out of the way of the ascending heat, and that it be hung to the under parts of the metal fixtures to which it is attached, so that it will not be bent or kinked out of the natural curve which it would form if it were suspended by the ends. If these conditions are complied with, it is claimed that the tubing will last as long as the life of the chandelier itself.

It is proposed by the inventor, who is secretary of the Union Tubing Co., to give permits to use these designs, and furnish them at a mere nominal sum, to all who purchase tubing of them for the purpose specified.

HOW TO DRAW A STRAIGHT LINE.*

III.

BEFORE leaving the Peaucellier cell and its modifications, I must point out another important property they possess besides that of furnishing us with exact rectilinear motion. We have seen that our simplest linkwork enables us to describe a circle of any radius, and if we wished to describe one of ten miles radius the proper course would be to have a ten mile link, but as that would be, to say the least, cumbrous, it is satisfactory to know that we can effect our purpose with a much smaller apparatus. When the Peaucellier

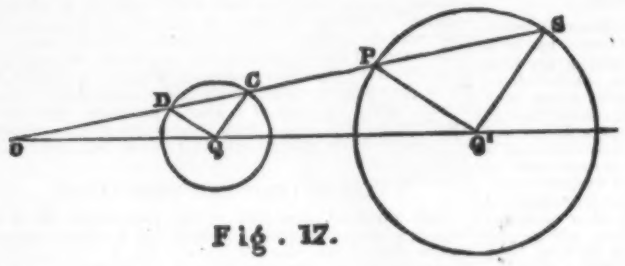


Fig. 17.

cell is mounted for the purpose of describing a straight line, as I told you, the distance between the fixed pivots must be the same as the length of the "extra" link. If this distance be not the same we shall not get straight lines described by the pencil, but circles. If the difference be slight the circles described will be of enormous magnitude, decreasing in size as the difference increases. If the distance, Q O, Fig. 6, be made greater than Q C, the convexity of the portion of the circle described by the pencil (for if the circles are large it will of course be only a portion which is described) will be towards O, if less, the concavity. To a mathematician, who knows that the inverse of a circle is a circle, this will be

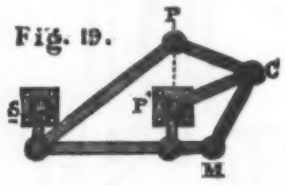


Fig. 19.

clear, but it may not be amiss to give here a short proof of the proposition.

In Fig. 17, let the centers Q Q′ of the two circles be at distances from O proportional to the radii of the circles. If then O D C P S be any straight line through O, D Q will be parallel to P Q′, and C Q to S Q′, and O D will bear the same proportion to O P that O Q does to O Q′. Now, considering the proof we gave in connection with Fig. 7, it will be clear that the product O D · O C is constant, and therefore since O P bears a constant ratio to O D, O P · O C is constant. That is, if O P · O C is constant, and C describes a circle about Q′, P will describe one about Q′. Taking

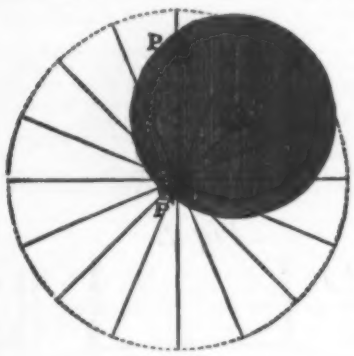


FIG. 20.

then O, C, and P as the O, C, and P of the Peaucellier cell in Fig. 7, we see how P comes to describe a circle.

It is hardly necessary for me to state the importance of the Peaucellier compass in the mechanical arts for drawing circles of large radius. Of course the various modifications of the "cell" I have described may all be employed for this purpose. The models exhibited by the Conservatoire and M. Breguet are furnished with sliding pivots for the purpose of varying the distance between O and Q, and thus getting circles of any radius.

My attention was first called to these linkworks by the lecture of Prof. Sylvester, to which I have referred. A pas-

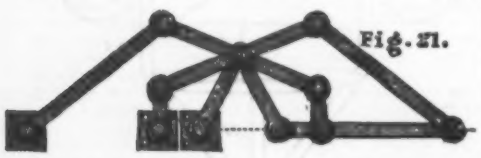


Fig. 21.

sage in that lecture in which it was stated that there were probably other forms of seven-link parallel motions besides M. Peaucellier's, then the only one known, led me to investigate the subject, and I succeeded in obtaining some new parallel motions of an entirely different character to that of M. Peaucellier. I shall bring two of these to your notice as the investigation of them will lead us to consider some other linkworks of importance.

If I take two kites, one twice as big as the other, such that the long links of each are twice the length of the short ones, and make one long link of the small kite lie on a short one of the large, and a short one of the small on a long one of the large, and then amalgamate the coincident links, I shall get the linkage shown in Fig. 18.

* Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A. Continued from p. 1356.

The important property of this linkage is that, although we can, by moving the links about, make the points P and P′ approach to or recede from each other, the imaginary line joining them is always perpendicular to that drawn through the pivots on the bottom link L M. It follows that if either of the pivots P or P′ be fixed, and the links L M be made to move so as to always remain parallel to a fixed line, the other point will describe a straight line perpendicular to the fixed line. Fig. 19 shows you the parallel motion made by fixing P′. It is unnecessary for me to point out how the parallelism of L M is preserved by adding the link S L; it is obvious from the figure. The straight line which is described

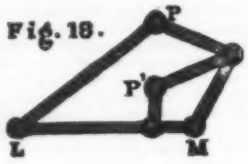


Fig. 18.

by the point P is perpendicular to the line joining the two fixed pivots; we can, however, without increasing the number of links, make a point on the linkworks describe a straight line inclined to the line S P at any angle, or rather we can, by substituting for the straight line P C a plane piece, get a number of points on that piece moving in every direction.

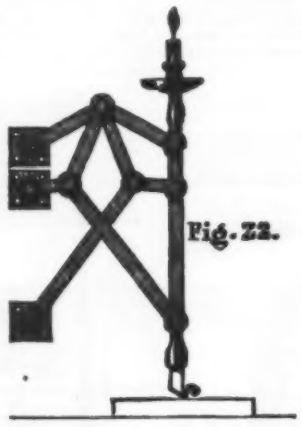


Fig. 22.

In Fig. 20, for simplicity, only the link C P′, and the new piece substituted for the link P C are shown. The new piece is circular and has holes pierced in it all at the same distance—the same as the lengths P C and P′ C—from C. Now we have seen from Fig. 19 that P moves in a vertical straight line, the distance P C in Fig. 20, being the same as it was in Fig. 19; but from a well known property of a circle, if H be any one of the holes pierced in the piece, the angle H P′ P is constant, thus the straight line H P′ is fixed in position, and H moves along it: similarly all the other holes move along in straight lines, passing through the fixed pivot P′, and we get straight line motion distributed in all directions. This species of motion is called by Prof. Sylvester "tram-motion." It is worth noticing that the motion of the circular disk is the same as it would have been if the dotted circle on it rolled inside the large dotted circle; we have, in fact, White's parallel motion reproduced by linkwork. Of course, if we only require motion in one direction, we may cut away all the disk except a portion forming a bent arm,

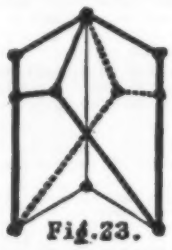


Fig. 23.

containing C, P, and the point which moves in the required direction.

The double kite of Fig. 18 may be employed to form some other useful linkworks. It is often necessary to have, not a single point, but a whole piece moving so that all points on it move in straight lines. I may instance the slide rests in lathes, traversing tables, punches, drills, drawbridges, etc. The double kite enables us to produce linkworks having this property. In the linkwork of Fig. 21, the construction of which will be at once appreciated if you understand the double kite, the horizontal link moves to and fro as if sliding in a fixed horizontal straight line. This form would possibly be useful as a girder for a drawbridge.

In the linkwork of Fig. 23, which is another combination of two double kites, the vertical link moves so that all its points move in horizontal straight lines. There is a modification of this linkwork which will, I think, be found inter-

FIG. 25.

esting. In the linkage in Fig. 23, which, if the thin links are removed, is a skeleton drawing of Fig. 22, let the dotted links be taken away and the thin ones be inserted; we then get a linkage which has the same property as that in Fig. 22, but it is seen in its new form to be the ordinary double par-

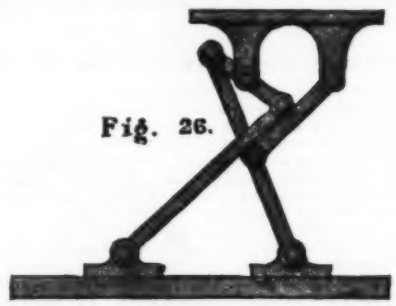


Fig. 26.

allel rule made on this plan with a slight modification. If the bottom ruler be held horizontal the top moves vertically up and down the board, having no lateral movement.

While I am upon this sort of movement I may point out an apparatus exhibited in the Loan Collection by Prof. Tchebicheff which bears a strong likeness to a complicated camp-stool, the seat of which has a horizontal motion. The motion is not strictly rectilinear; the apparatus being, as will

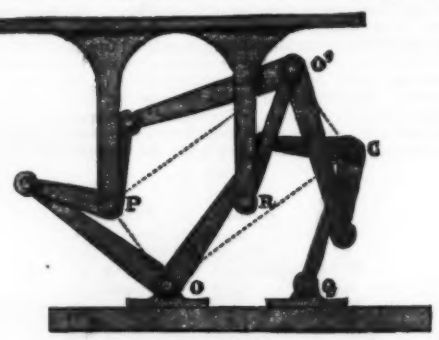


FIG. 27.

be seen by observing that the thin line in the figure is of invariable length, and a link might therefore be put where it is, a combination of two of the parallel motions of Prof. Tchebicheff given in Fig. 4, with some links added to keep the seat parallel with the base. The variation of the upper plane, from a strictly horizontal movement, is therefore double that of the tracer in the simple parallel motion.

Fig. 24 shows how a similar apparatus of much simpler construction employing the Tchebicheff approximate paral-

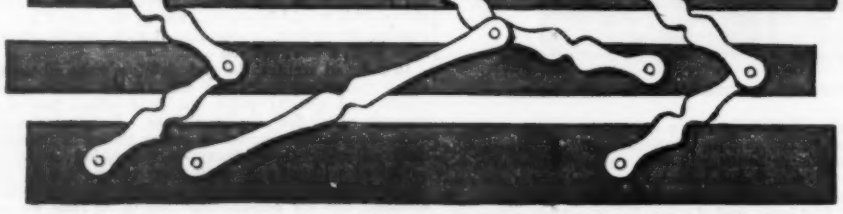


FIG. 24.

lel motion can be made. The lengths of the links forming the parallel motion have been given before (Fig. 4). The distance between the pivots on the moving seat is half that between the fixed pivots, and the length of the remaining link is one half that of the radial links.

An *exact* motion of the same description is shown in Fig. 27. O, C, O′, P are the four *foci* of the quadriplane shown in the figure in which the links are bent through a right angle, so that O C · O P is constant, and C O P a right angle. The focus O is pivoted to a fixed point, and C is made by means of the extra link Q C to move in a circle of which the

radius Q C is equal to the pivot distance O Q. P consequently moves in a straight line parallel to Q C, the five moving pieces thus far described constituting the Sylvester-Kempe parallel motion. To this are added the moving seat and the remaining link, R O', the pivot distances of which, P R and R O', are equal to O Q. The seat in consequence always remains parallel to Q O, and as P moves accurately in a horizontal straight line, every point on it will do so also. This apparatus might be used with advantage where a very smoothly-working traversing table is required.

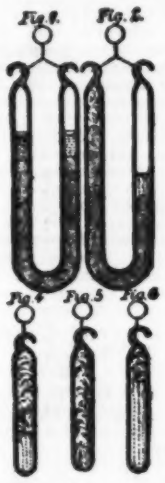
(To be continued.)

THE ELASTICITY AND DENSITY OF VAPORS.

Experiments on the Maximum Elasticity and Density of Vapors.

By MR. ALEX. MORTON.

FROM the experiments of M. V. Regnault on the elasticity of vapors, we learn that generally the more volatile the liquid, the more rapidly does the elasticity of its vapor increase with its heat. He has shown us that there are some vapors which do not continue so related, even within the range of his experiments, hence my use of the word "generally;" for instance, absolute alcohol and chloroform, although the one is considerably more volatile than the other, at about



302 deg. F., by the mercurial thermometer, they become exactly of the same elasticity; below that temperature the vapor of chloroform is greatest, therefore they may be said to cross each other at that point; but so far as his observations went, these vapors and that of bisulphide of carbon never appeared to approach, but rather departed from that of distilled water (steam), even at his highest temperatures. These curves are represented on drawing No. 2; the temperatures are measured horizontally, and the elasticities vertically from Regnault's tables in his "Memoires" I have referred to. Certain considerations led me to think that these vapors would approach and cross distilled water at temperatures within the range of experiment; and although it would not be difficult to construct vessels strong enough to withstand the great pressure they would be subjected to, still, to find a gauge which would register these pressures with any degree of accuracy would be next to impossible.

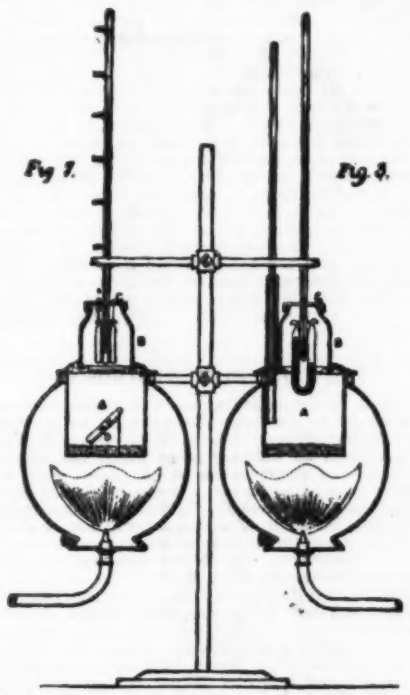
I am aware that Dr. Andrews has said in a recent paper to the Royal Society, that he has discovered a reliable gauge; but my chief object was to learn if at any temperature they would become of the same elasticity, irrespective of what that elasticity might be, and accordingly I made use of U-tubes, as shown by Figs. 1 and 2, annexed. The lower portion of these tubes, and so far up each limb, is filled with dry mercury. In the spaces above the mercury each end contains about fully one third of the respective liquids to be experimented with. The ends of the U-tube having been hermetically sealed cold, air occupies the spaces above the liquid.

The apparatus is now ready for experiment; and to perform it so that I should observe what took place in the time of heating, I constructed a hot air bath A, Fig. 3. This bath A, made of brass, was suspended from an ordinary stand within a common gas globe, which acted as a hot air casing for it. The upper end of this bath A consists of a small glass casing B, through which every change was easily noted as the experimental tubes were hung by fine wires to the cover C, so that they were constantly within my view. I used two mercurial thermometers, graduated to 600° Fahr. The lower one hangs within a slotted brass tube for protection, and was only used when testing the tubes on the first heating them; the upper one is naked, and so adjusted, between the limbs of a U-tube containing alcohol and chloroform, that the mercury stands level at 302° Fahr., which is the known crossing point of these two vapors, and the figures in this paper are from the readings of this naked thermometer so adjusted.

With a U-tube containing distilled water in one end and chloroform in the other end, suspended in the bath, as the heat increases the chloroform evaporates, and compresses the distilled water up into its own end, as represented by Fig. 2. The air in that end very shortly becomes so reduced in bulk as to be imperceptible.

On continuing to raise the temperature, the chloroform at times suddenly attains the "Cagniard de la Tour" state; but this does not affect the experiment, as the distilled water begins to evaporate so soon as a certain temperature is reached, and the elasticity of its vapor compresses the chloroform vapor up into its own end, exactly as the water had been at the lower temperatures. The chloroform is now liquid, so far as that term expresses its state; it has therefore been converted from vapor into liquid by compression. To ascertain if it be liquid, lower the temperature below the crossing point, and you will observe it begin to evaporate exactly as it did on first heating. By adjusting the gas flame you can catch and maintain the mercury exactly level in both limbs; and I find after repeated experiments, that that temperature is about 497° Fahr., and this point changes but little, if any, although you double the quantity of chloroform. This is a simple and beautiful experiment—simple, because it is easy to get a bent glass tube to withstand the moderate pressure, and you can with perfect freedom examine every change with an unscreened face; beautiful, because it shows clearly a liquid's conversion into vapor, and afterwards its compression into a liquid again imperceptibly, not even a cloud or mist to herald the change, nor the faintest trace of demarcation. With another similar U-tube containing alcohol absolute and distilled water, I have found that the crossing temperature is a little above that of melting lead, but still far short of that of melting zinc. This is a very difficult experiment, and many of the tubes have burst with great violence after the lead melted.

With another similar U-tube containing bisulphide of carbon and distilled water, I find that the crossing temperature is about 540° Fahr. The three liquids—pure chloroform absolute alcohol, and bisulphide of carbon—have all reached the "Cagniard de la Tour" state below the crossing temperature, and before the elasticity of distilled water has become equal with that from their vapors at the same temperature, but chloroform and alcohol become of an equal elasticity, and cross each other, as we know, at 302° Fahr.; therefore, neither have changed, as both ends of the tube contain the boiling liquids with the mercury level. I find bisulphide of carbon and alcohol behave in the same manner, and that the crossing temperature for these vapors is about 360° Fahr. With the U-tube containing chloroform and distilled water in place, on continuing the heat up to and a little above that of melting lead, I find the elasticity of the vapor of water is so great as to compress the chloroform into certainly less than one and a half times its original volume, although before the change it had become about twice its bulk. I may here explain that the reason why I preferred the U-tubes sealed cold, containing air in the spaces, to those sealed when the liquids within them were thoroughly boiling, is simply because the latter required greater differences of temperature above and below the crossing points to start the liquids into ebullition. When liquids nearly devoid of air are under great pressure, they persist in maintaining the state they are in, and consequently a greater difference of temperature is required; moreover, when ebullition does begin, you cannot adjust the heat in time to catch the mercury in both limbs level, the elasticity of the vapor of one always overpowers that of the other, and to ascertain the crossing temperature you can only take the mean of the differences. Even with air in the spaces there are some liquids more persistent than others, and when the mercury cannot be caught exactly level, I have taken the mean for the crossing temperature. If the tubes be much tapered in sealing, this also seems to retard ebullition. I may conclude this part of my paper by remarking that these experiments



with the chloroform, alcohol, and bisulphide of carbon show, I think satisfactorily, that the vapors of these more volatile liquids do approach, and even cross that of distilled water, at no great increase of temperature above that of Regnault's experiment, as I had anticipated. The vapor of chloroform decomposes and lines the inside of the tube with a black film, rapidly at the higher temperatures, when mixed with the vapor of mercury; but alcohol appears as pure and transparent after repeated experiments with the same tube as it did on first heating.

From the low elasticity of spirits of turpentine, I was led to try if I could reach the maximum of its elasticity, and with that in view I got prepared a U-tube somewhat similar to those just described. One limb contained the liquid and other air; that end filled with air, above the mercury, was drawn out to a small diameter before it was sealed. The compression of the air was intended to measure the maximum elasticity of the vapor in the other limb of the U-tube. On making the experiment, I found that although the tube itself remained perfectly transparent, the liquid colored very rapidly at the higher temperatures, and that at about the temperature of melting zinc it explodes, producing a column of smoke, exactly as if it had been ignited gunpowder. I here use the word "explodes" in contradistinction to "bursts." Before the explosion the liquid had disappeared and become dark brown vapor.

To enable me to perform my experiments at higher temperatures and pressures, I used single glass tubes, as represented by Figs. 4, 5, and 6; these have often withstood the elasticity of the vapor of water near the temperature of melting antimony. I have had no difficulty whatever in getting tubes of these proportions to withstand the elasticity of steam due to a temperature considerably above that of melting zinc. If one of these tubes be half filled, or nearly so, with any liquid I have used, and that tube hermetically sealed when the liquid within it is thoroughly boiling, I find on hanging it in the air-bath that as the heat augments, the liquid continues to evaporate and increase in bulk, until the whole tube appears filled with the boiling liquid. One would have thought that the liquid would have become gradually less as the heat increased, but the contrary takes place. The liquid appears to expand, slowly at first, but more rapidly at the higher temperatures, until the whole tube appears filled with the boiling liquid; and if not previous, at that instant the tube becomes completely filled, the "Cagniard de la Tour" state comes on. If the tubes be only one-fourth, or less, filled with liquid, it invariably becomes less until the last globule of liquid has disappeared. I am not aware that this phenomenon has ever been pointed out, and as it has particularly attracted my attention I now record it.

Three of these tubes, containing one-fourth, one-third, and one-half of liquid chloroform, are hung in a circle (Fig. 7) around and in contact with the naked thermometer; on first heating these, the one containing the greatest quantity appears first in this peculiar state referred to, but the difference of temperature between the first and last is only a few degrees; and for pure chloroform, by adjusting the gas flame so as to maintain a uniform constant temperature, each tube may be kept, for minutes at least, apparently full of a cloud of smoke, of a color resembling that which arises from burning coal. If the temperature be lowered one-half of a degree Fahrenheit, those tubes containing one-half and one-third of liquid appear full of the boiling liquid; if it be raised to the same distance above that particular temperature, these tubes appear filled with a pure homogeneous vapor. The particular point for chloroform, I find, varies from 510° to 520° Fahr.

With these tubes containing alcohol of the same quantities as the chloroform, I find that the cloud or mist is pure white, and that that temperature, when the cloud is constant, varies from about 480° to 490° Fahr. The alcohol behaves otherwise as the chloroform did, only it attains that peculiar state at a much lower temperature. With three similar tubes containing spirits of turpentine, the liquid, as I have before said, changes color and the vapor partakes of the color of the liquid, viz., dark brown. At about the temperature of melting zinc, the whole three are apparently in this peculiar state; but this experiment requires that the heat should be quickly augmented, and that instant when the liquid becomes vapor, the gas flame should be turned entirely off, and a current of cold air admitted to surround the experimental apparatus. If the heat be slowly augmented, the vapor explodes with great violence at about that temperature. A wire gauze screen on the face, and even gloves, cannot be too often recommended as essential in such experiments, as you are not certain but what the movement of your foot may cause a vibration sufficient to induce the explosion.

Many tubes containing other liquids have burst when my face was within a few inches of the experimental apparatus; and although these vapors were so high in temperature just the moment before they burst the tubes, the feeling at the instant was that of a blast of ice-cold air.

With ether and bisulphide of carbon, the first becomes vapor at the temperature of about 400° Fahr., and the latter at about 500° Fahr. The cloud or mist with the former is first pale blue, changing into grey as the vapor returns to liquid. The cloud or mist formed by the latter partakes of the color of the liquid, which with a first heating becomes pale brown, although pure and void of color originally. A second heating seems to double the intensity of the color.

I shall here describe experiments with two series of three tubes, each containing ether and bisulphide of carbon, sealed when thoroughly boiling.

On first heating the ether, that tube containing one-third of liquid changed first, then the one containing one-half, and lastly the one containing one-fourth. After the three tubes had been three times through the process, the tube containing one-half invariably went off first (*i.e.*, it went into the peculiar state referred to), the one-third next, and lastly the one-fourth; and in cooling they followed the inverse order—that is, the tube which first went off was last in returning; and the temperature for the ether I find thus—the tube containing one-half liquid when cold became apparently gaseous at 375° Fahr.; those containing one-third and one-fourth went off nearly simultaneously at 405° Fahr.

The bisulphide of carbon series at first, and having been twice through the process, went off as follows: The tube containing one-fourth of liquid (when cold) at 475° Fahr., that containing one-third at 495° Fahr., and, lastly, that containing one-half at 515° Fahr. You will observe that the tube containing the greatest quantity of liquid ether invariably went off first, whereas that containing the greatest quality of liquid bisulphide of carbon invariably last.

With distilled water boiled and sealed with glass tubes similar to those shown by Figs. 4, 5, and 6, only one experiment can be satisfactorily performed with a tube, because the vapor in returning to liquid decomposes the glass so rapidly that its transparency is lost. However, I have learned that the decomposition takes place chiefly when the vapor is returning to its original liquid state. Having ascertained this, I had the hot-air bath raised to the temperature of melting zinc before introducing the experimental tubes; they therefore very shortly became of a high temperature; and as the gas flame was burning constantly, they increased in temperature until the liquid had entirely disappeared; they never lost their transparency, and I could tell exactly when that peculiar point referred to was reached. The decomposition of the glass apparently takes place whilst the vapor is condensing. If no portion of the vapor ever condenses during the experiment, the tube remains as pure and transparent as with any other liquid I have used; but if once cooled, the tube becomes perfectly opaque. I find that the cloud somewhat resembles that of alcohol as to color, being nearly white, and that the cloudy state temperature is a little above that of melting zinc.

Seeing that only one experiment can be performed with glass tubes containing water, I had necessarily to prepare a great number of these, containing from one-fourth to one-half of liquid, and invariably those containing the least quantity of liquid first attained that peculiar state before the zinc had melted; but those containing one-third of liquid became perfectly gaseous after the zinc had melted. These tubes, when prepared for experiment, had a spiral cutting of sheet zinc around them, and they were in contact with a small piece of antimony suspended by a fine brass wire. I had very few single tubes which burst during my experiments with the distilled water.

I have said that the vapor of chloroform decomposes rapidly when mixed with the vapor of mercury at high temperatures; but with clean single tubes containing pure chloroform, sealed when boiling, you may use the same tube over and over again, a dozen times at least, if merely heated to, say, 530° Fahr., and still you find the same quantity of liquid pure and transparent as at first within it; but if any of the tubes be once raised to between the

temperature of melting zinc and melting antimony, the liquid becomes deeply colored a dark brown, and shortly afterwards explodes.

(*To be continued.*)

THE CYCLOSCOPE.

It is well known that if a mirror be attached to a vibrating tuning-fork, and a point of light which moves uniformly in a plane at right angles to that in which the fork is vibrating be reflected from this mirror, the image will be an ordinary single wave. Again, if a series of luminous points move uniformly with such velocity that a point passes over two intervals during an odd number of vibrations of the fork, the two waves overlap and produce a double figure of the form of a series of figures-of eight. Extending these principles, Professor McLeod and Lieutenant G. S. Clarke have recently constructed an ingenious apparatus which has been described before the Royal Society under the name of the cycloscope. Equidistant perforations are made in a circle on a disk, which is attached to a rotating axis, and the light passing through these apertures falls upon a vibrating tuning fork of known period, whence it is reflected on to a screen; and from the shape of this reflected image the rate of rotation can be deduced. Hence the cycloscope promises to become of much value in determining the speed of machinery. On the contrary, if the speed at which the cylinder rotates be known, the pitch of the tuning-fork may be ascertained.—*Proc. Roy. Soc., Popular Science Review.*

THE ELECTRIC CANDLE.

Experiments have been recently conducted at the West India Docks with the view of testing the illuminating power of the so-called electric candle devised by M. Paul Jablochkoff. This simple means of producing a steady electric light consists in placing two carbon pencils side by side, but separated by a bar of a composition called "kaolin." On the passage of the current the carbons slowly burn down, and the kaolin is consumed by the heat at exactly the same rate. The carbons are thus kept always at the same distance apart, and the light playing between them is thus rendered constant without the aid of complex regulators. In the experiments at the West India Docks the current was produced by a magneto-electric machine, worked by a small steam-engine, and the results are described as having been eminently satisfactory. For lights of small and medium size, an apparatus of even greater simplicity may be employed, the carbon points being dispensed with and nothing used beyond a piece of the so-called kaolin held between the electrodes. But M. Jablochkoff's prime improvement, which promises to greatly extend the use of the electric light, consists in his ability to divide the current, so as to supply several candles placed in the same circuit, each with its own coil. These candles may be of various degrees of illuminating power, and may be lighted or extinguished separately. In short, the electricity appears to be under such control, that it might be generated in some central establishment and laid on through wires to the several centers of illumination, just as freely as gas is at present distributed through pipes to any number of burners. MM. Denayrouze and Jablochkoff,, who have employed the light in Paris, described their process before the French Academy of Sciences.—*Comptes Rendus.*

INFLUENCE OF LIGHT ON THE ELECTRIC RESISTANCE OF METALS.

Some two or three years ago we were startled by the announcement that the electric conductivity of selenium is capable of being affected by light. It has, however, been since determined that not only selenium but also the allied element tellurium has its electric resistance diminished after exposure to luminous rays. Desirous of determining whether other bodies are similarly affected, Dr. Börnstein has carried out some interesting researches in the Physical Institute at Heidelberg. His results are published in a paper which has been translated by Mr. R. E. Day, in the "Philosophical Magazine" (June, 1877, p. 481). Gold, silver, and platinum are the only metals which Dr. Börnstein has yet examined, but as he finds that all these are sensitive to light, he is inclined to infer that the property, so far from being exceptional, is one enjoyed in greater or less measure by all metals. In the case of selenium and tellurium, it has been suggested that the alteration of resistance is due to the action of calorific rather than luminous rays. But no such objection can be urged against the experiments with the noble metals. In fact, the resistance of these metals increases with the temperature, so that when it is found that on exposure to direct light the resistance is diminished, it is clear that heat can have nothing to do with causing such a change. Heat, indeed, tends to mask the effects of light, and the diminution of resistance is therefore a differential effect; an effect representing the difference between the increase of resistance consequent on rise of temperature, and the decrease of resistance consequent on rise of temperature, and the decrease of resistance due to the action of light Dr. Börnstein's experiments therefore, show beyond question that the electric conductivity of the noble metals is exalted, or their resistance diminished, by the direct effect of luminous rays.

MAGNETIZATION OF POLARIZED LIGHT.

It was Faraday who first showed, by a series of famous experiments, that magnetism is capable of exerting a remarkable influence on light which has undergone that physical change which is called polarization. When a beam of plane-polarized light is transmitted through certain transparent media placed between the poles of an electro-magnet, the plane of polarization is rotated in this direction or in that, according to the course which the electrical current is caused to take in circulating through the coils of the magnet. Dr. J. Kerr, of Glasgow, reflecting on this fact and others of a kindred nature, was led to institute experiments with the view of determining whether the beam of plane-polarized light, when reflected from an iron surface highly magnetized, would have its plane of polarization twisted in the act of reflection. A paper describing his experiments in detail, and corroborating his assumption, will be found in a recent number of the "Philosophical Magazine" (May, 1877, p. 321). An upright electro-magnet of horse-shoe shape had its polar surfaces polished so as to form reflecting planes. Moreover, a sub-magnet was employed to concentrate the magnetic force, by inductive action, upon the surface of the iron mirror. A beam of light polarized by transmission through a Nicol's prism was incident at various angles upon this mirror, and after reflection was analyzed by passing through a second Nicol. Let these prisms be crossed so as to extinguish the light, and then send a current through the electro-magnet; as soon as the iron mirror becomes magnetized the obscuration is diminished, showing, of course, that the plane of polarization has been turned. The direction in which it is turned is contrary to that of the magnetizing current. If, for example, the magnetized iron mirror becomes a true south pole, then the plane of polarization is turned to the right hand; if the current be reversed, so that the magnet becomes a north pole, the rotation is to the left. It should be remarked that Dr. Kerr, following Sir W. Thomson, uses in this respect a different nomenclature from that popularly employed. His true south pole would be one that points, on the whole, towards the north; that is to say, what most people would call the north pole of the magnet. It seems reasonable enough that the great earth-magnet should have its south pole in the southern, and its north pole in the northern hemisphere. But if artificial magnets are to have their poles similarly named to the like poles of the earth, then (remembering that opposite poles attract) we shall have to call the true south pole of the magnet that which points to the magnetic north; in other words, we reverse the common nomenclature. It is in the highest degree desirable that uniformity should be attained, and confusion prevented, in the use of such common terms. Indeed, it has been suggested to abolish such phrases as the north and south ends of the needle, and to substitute others free from ambiguity.

METHOD OF DETERMINING INDEX OF REFRACTION.

It is by no means an easy matter to determine the index of refraction of a solid substance, say, of a gem-stone, or other transparent mineral. And yet the refractive power is unquestionably an important physical characteristic, and one which would no doubt be often recorded if it could be measured with readiness. Mr. H. C. Sorby is therefore to be congratulated on placing in our hands a method of extremely simple application ("Mineral. Magazine," No. 4, 1877, p. 97). He takes a small piece of the transparent substance, having flat parallel sides, and placing it on the stage of the microscope, finds the thickness of the fragment, and also the extent to which the focal length of the object-glass is displaced by looking through this thickness of the medium. These measurements are effected by attaching to the body of the microscope a small graduated scale with a vernier, enabling the observer to read off to the thousandth of an inch. To make the two required measurements is but the work of a few moments, and having obtained these data, the index of refraction is immediately calculated; it is, in fact, equal to the thickness divided by the difference between the thickness and the increase of focal length. By this method Mr. Sorby has already determined the index of refraction of a large number of substances, and has obtained some interesting results. For example, he finds that the mineral called proustite, or light-red silver-ore, has a refractive exponent as high as 3·28; much higher indeed than that of any substance previously examined. It has been proposed at various times to construct microscopic lenses of diamond or sapphire, but although these gems have very high refractive indices, they are far exceeded in this respect by proustite. In fact, were it possible to construct a lens of this rare mineral, it would have about twice the magnifying power of a lens of flint-glass of equal curvature. The index of flint-glass is certainly not higher than 1·6, which, as we have seen, is only about half that of proustite.

THE HALF-PRISM SPECTROSCOPE.

Instead of the ordinary isosceles prisms used in our spectroscopes Mr. Christie, of Greenwich, has recently suggested the introduction of half-prisms. By a "half-prism" we understand an ordinary prism which has been cut in half by a plane perpendicular to the base. Such a prism possesses peculiar advantages. Suppose two slightly divergent rays of monochromatic light to pass through an ordinary prism, the angle of separation will not be altered, so that the rays emerge at the same angle to each other which they had at entrance. But if two such rays fall perpendicularly upon a face of the half-prism, the deviation of one ray in traversing the glass will be much greater than that of the other, and they will consequently emerge separated by a much wider angle. In fact, the half-prism is really equivalent to a magnifier with cylindrical lenses. Mr. Christie has had several half-prism direct-vision spectroscopes successfully constructed by Mr. A. Hilger, and has described them before the Physical and Astronomical Societies ("Astron. Register," May, 1877, p. 111). Half a prism of flint-glass is cemented to a half-prism of crown glass, and a combination giving great dispersion is thus obtained. If the double sodium line be viewed through such a spectroscope, the two lines will be seen widely separated, and in consequence of the dispersion they will be broader than with an ordinary instrument. With two such half-prisms a dispersion equal to that of ten compound prisms has been obtained; and this, too, without any loss of definition. Moreover, either great dispersion or great purity of spectrum can be obtained at pleasure, according as the light is allowed to fall first upon the perpendicular or upon the oblique face of the half-prism. Hence by reversing a train of half-prisms, so that the light falls on the opposite side, a comparatively short spectrum is obtained, but one of great purity. Indeed, without use of collimating lenses or slit, the sodium lines may be seen in an ordinary unsalted candle-flame, and Fraunhofer's lines may be seen in the solar spectrum.

THE ELECTRIC CONDUCTIVITY OF WATER.

It seems at first sight curious that in the case of so common a substance as water there should be any doubt among physicists as to the power which the liquid possesses of conducting electricity. Not that the subject has by any means been neglected, as the labors of Magnus, Pouillet, Becquerel, Oberbeck, Rosetti, and Quincke abundantly testify. But the results obtained by these several investigators differ so widely among themselves that it is not easy to know which are to be accepted. The figures brought out by Pouillet, for example, are sixty times greater than those deduced by Magnus. Professor Kohlrausch has, therefore, thought it high time that the subject was settled; and after a most carefully conducted investigation, he has published the results of his inquiry in Poggendorf's "Annalen" (*Ergänzungsb*, iii, 1877, p. 1). Many substances have their electric conductivity affected to a remarkable extent by the presence of impurities, even when these are present in only minute proportion. Matthiessen showed that copper, for instance, has its conductivity lowered 40 per cent. by presence of a mere trace of arsenic. In like manner Kohlrausch has found it necessary to pay scrupulous attention to the purity of the water which he examined, and indeed the discrepancies among the results of previous inquiries may probably be explained by inattention to this point. Having prepared the liquid in as pure a state as the resources of the laboratory can permit, Kohlrausch tested the conductivity by examining a shell of this water enclosed between two hemispherical vessels of platinum nearly fitting one into the other, and serving as electrodes. On passing a current of electricity through this arrangement, it was found that the pure water offered remarkable resistance; in fact its conductivity was only $\frac{1}{150}$ of that assigned to it by Pouillet. Rain, which is, of course, the purest natural form of water, conducted electricity twenty five times better than the artificially-purified liquid which served as a standard.

LONGITUDE BY TELEGRAPH.

Commander Perrier read a paper at the last meeting of the Geographical Society of Paris, on the determination of the longitude of Algiers by telegraphy. The exact longitude is 2° 50′·21 east from Paris, the probable error being only 0″·01. The time required for the transmission of the electricity from Paris to Marseilles was found to be only $\frac{1}{150}$ of a second; the distance between these two cities being 863 kilometres, it shows that the velocity of the electricity was not less than 46,000 kilometres per second. Similar experiments tried on the submarine cable between Algiers and Marseilles proved that the time required to travel was $\frac{23}{100}$ of a second; for a distance of 926 kilometres this shows a velocity of only 4,000 kilometres. But the battery used for signalling in the aërial line was composed of 100 elements, and only ten elements were used in the sub-Mediterranean cable. The triangulation of Algeria is an accomplished fact, and the calculations will be finished two or three months hence. When the operations shall have been completed it will be possible to know the exact length of an arc of meridian passing through the Paris Observatory, and extending from Shetland to Laghouat. The amplitude will be exactly 30°. An arc of parallel will be measured, also extending from Nemours on the Marocco frontier to Bona, in the vicinity of Tunis. The mean latitude will be 36° and amplitude 10°.—*Nature.*

ON IRON AS A NATURAL CONSTITUENT OF WINES.

By C. R. ALDER WRIGHT, D.Sc. (Lond.).
Lecture on Chemistry in St. Mary's Hospital Medical School.

That iron sometimes exists in the ash left on evaporating wines to dryness and incinerating the residue has long been known; but in many cases it is not so certain as to how far this iron was pre-contained in the grape-juice employed, and how far it was derived from substances added to the wine during the process of manufacture—notably from gypsum used in the "plastering" process adopted in some cases, or from impurities in the glucose, etc., added to the grape-juice so as to obtain a larger alcoholic yield. The wine employed in the following experiments was obtained direct from Messrs. Auld, Burton & Co., of the Auldana Vineyards, South Australia. It had been grown and made under the superintendence of one member of the firm in Australia, imported in cask, and bottled in the London stores of the firm, so that, not having passed through the hands of agents, its genuineness and freedom from sophistication were as well guaranteed as such matters can be.

The following numbers represent the analytical figures obtained with two varieties of Auldana wine, distinguished as "Ruby" and "White." The alcoholic strengths are necessarily only close approximations, as each wine was heavily charged with volatile compound ethers, imparting a marked and peculiar bouquet, and communicating to the wines a higher exhilarating effect than that simply due to the actual alcohol present. It is noteworthy that the sulphates present are very low, and in less quantity than the potassium bitartrate, which indicates that no plastering had been adopted in either case, whilst the low percentage of grape-sugar shows that the fermentative process had been carried out very completely.

The numbers represent the grammes of each constituent per 100 c.c. (or grains per 100 fluid grains).

	Ruby.	White.
Sp. gr. at 60° Fah.	0·9874	0·9884
Alcohol and volatile compound ethers	10·500	12·000
Grape-sugar	0·207	0·248
Anhydrous sulphuric acid (SO_3)	0·043	0·048
Do phosphoric acid (P_2O_5)	0·020	0·023
Chlorine (Cl)	0·015	0·015
Silica (SiO_2)	0·007	0·008
Potassium bitartrate*	0·123	0·136
Potash (K_2O) in other conditions than as bitartrate	0·127	0·106
Soda (Na_2O)	0·026	0·023
Ammonia (NH_3)	0·007	0·010
Lime (CaO)	0·016	0·014
Alumina	traces	traces
Magnesia (MgO)	0·025	0·027
Protoxide of iron (FeO)	0.001	0·001
Volatile acids (calculated as acetic acid)	0·214	0·108
Glycerin, dextrin, tannin, vegetable acids, etc.	2·375	2·657
Water (by difference)	86·294	84·576
	100·000	100·000
Total solid constituents	2·992	3·316
Total free acid (calculated as tartaric acid)	0·704	0·652
Total potash present (K_2O)	0·158	0·140
Matters left on in incineration:		
Alkaline portion calculated as K_2CO_3	0·131	0·089
Neutral substances soluble in water	0·150	0·143
Matters soluble in water	0·060	0·076
Total	0·341	0·308

The determination of the state in which the iron present existed being of some interest, the following experiments were made, a large bulk of wine being employed (upwards of 10 litres of each kind):

DETERMINATION OF TOTAL IRON PRESENT.

Known bulks were evaporated to a syrup and charred; the matters soluble in water were dissolved out and the residue burnt white; finally, the total iron in the aqueous solution and ashes jointly was determined; amongst other processes,

* Containing of K_2O: Ruby, 0·061; White, 0·064.

colorimetric tests—by comparison in Nesslerising cylinders with a weak standard iron solution, by means of ferrocyanide of potassium—were employed, and with fairly satisfactory results. The numbers deduced from several accordant observations gave the following average amounts, the iron being calculated as FeO:

Ruby.	White.
0·00130	0·00130

each wine containing sensibly the same amount of iron.

That the iron was not present as a salt of the peroxide was evident from the circumstance that each wine contained œnotannin, striking a bottle-green tint with ferric salts; moreover, no ferric reaction could be obtained with ferro- and sulpho-cyanides. On the other hand, ferri-cyanides gave on standing a blue precipitate in each case, showing the presence of soluble ferrous salts. In order to see if iron in other forms of combination (*e. g.*, analogous to blood-hæmatin) was present, known large bulks of wine were precipitated by acetate of lead, whereby almost complete decolorization ensued; the precipitates were well washed, dried, and incinerated, the lead separated from the nitro-hydrochloric solution of the ashes by pure sulphuric acid, and the iron in the filtrate determined colorimetrically. In this way the following numbers were obtained:

	Ruby.	White.
Iron in lead precipitate (calculated as FeO)	0·00060	0·00020

Hence it would seem that in the case of the ruby wine nearly one-half of the total iron present was carried down in the lead precipitate, whilst in the white wine only about one-sixth was thus precipitated. The experiments described below, however, indicate that the iron thus precipitated must have been carried down by a kind of mordanting action with the coloring matter, more being, of course, thrown down with the most colored wine, and not that the iron was really an integral part of the coloring-matter itself.

To see if any colloid iron compound was present, the wines were dialyzed through clean bladders immersed in about 5 parts of distilled water for 1 part of wine. Every day or two days the water was changed. For the first two or three changes much iron was contained in the diffusate, but it soon began to diminish, and after five days the diffusate contained only very small quantities of iron. The residual fluids in the bladder were then evaporated to dryness, and the residues incinerated and the iron determined. In this way there was found—

	Ruby.	White.
Iron remaining in bladders after ten days' dialysis (calculated as FeO)	0·00018	0·00016
Iron originally present	0·00130	0·00130

So that 85 per cent. of the iron had dialyzed away in the case of the ruby, and 88 per cent. with the white wine. The small residual amount doubtless was due to the dialysis not being carried to the absolute limit.

A clear proof that the iron contained was practically wholly present as a salt of protoxide was furnished by determining the amount of iron contained in the blue precipitate thrown down by ferricyanide of potassium on standing after washing. The quantities thus obtained (after deducting the iron in the precipitate due to the ferri-cyanogen) were—

	Ruby.	White.
Iron precipitated by ferri cyanide (calculated as FeO)	0·00146	0·00146

The excess thus found over the total iron previously obtained (0·00130 in each case) is doubtless due to a loss of a little iron during the incineration process and other experimental errors.

On examining the analytical figures given above it is manifest that the potash and soda of the wines are in a greater quantity than would suffice to neutralize the sulphuric and phosphoric acid and the chlorate, so that the ferrous oxide present can hardly have been contained as an inorganic salt; nor could it have been present as a tartrate, since presumably the tartaric acid would be wholly contained in the form of potassium bitartrate, much more potash being present than corresponds to the cream of tartar obtainable from the wine by precipitation with alcohol and ether, and the wine being of acid reaction. Hence the iron must have been present as a ferrous organic salt, probably malate or acetate.

Two circumstances are noteworthy as connected with the presence of iron in what there is every reason to believe were samples of wholly unsophisticated wine: first, that contact of the grape-juice with ironwork of any kind is studiously avoided, the crushing of the grapes being effected by means of wooden rollers covered with felt, the fermenting-vats, etc., being constructed of slate; secondly, that the soil of the Auldana vineyard is exceptionally ferruginous, the general petrological character of the district being decomposed clay-slate and limestone, with frequent deposits of ironstone. That iron is taken up from the soil by trees and vegetation generally, in minute quantity, is rendered certain from the constant occurrence of traces at least of that metal in the ashes of plants and vegetables of nearly all kinds; so that there is no inherent improbability, but rather the reverse, in the notion that the highly ferruginous soil of the Auldana vineyard, together possibly with climatic peculiarities, is the cause of the occurrence of iron in the finished wine. On the other hand, the circumstance that almost identical values were obtained with each kind of wine (the white being of 1872 vintage and the ruby of 1873) renders it improbable that the source of the iron is outside of the grape juice; for the grapes used for each kind of wine are the same, the differences being that in the production of the ruby wines the skins are fermented with the juice, whilst in the manufacture of the white wine they are previously carefully removed, and that the maturation and refining are carried out under slightly different conditions as to temperature, etc.

It deserves noticed that the Auldana wines, being thus apparently ferruginous from purely natural causes, and containing considerable amounts of phosphoric acid, are to some extent analogous to "lactophosphate of iron," "Parrish's Chemical Food," and the like; not improbably, therefore, they will be found valuable from a medicinal point of view as highly agreeable tonics.

AIR IN SEA WATER.

AT a meeting of the Royal Society of Edinburgh, June 4, 1877, Mr. J. Y. Buchanan, Chemist of the Challenger Expedition, read a paper on "*Air dissolved in Sea Water.*" He showed the water bottle which he invented, and which was used for collecting the "intermediate waters." It consists of a metal cylinder with tap at each end, connected by a metal rod bearing a flap, which falls into position when the bottle ceases to descend and is being "hove in;" the pressure of the water rushing past it causes it to descend and shut the taps, thus enclosing the desired sample of water. The apparatus for boiling out the oxygen and nitrogen, and that for determining the carbonic acid, were illustrated by diagrams, which also afforded an idea of the arrangements inside the miniature laboratory on board the Challenger. The general results of investigations made went to show that while the absolute amount of oxygen and nitrogen capable of being dissolved is less in the case of sea water than in that of fresh water, the proportion between the amounts of the two gases dissolved remains nearly the same; that the absolute amount dissolved both of permanent gas and of carbonic acid depends on the temperature; that in no case is there more gas dissolved in water taken from any depth than it would be capable of absorbing from the atmosphere in regions where the same temperature prevails at the surface; that, in fact, the water at great depths preserves all the physical properties which it had when it left the surface, including temperature, specific gravity, and gaseous contents (with the exception of the proportion of oxygen). The belief in the existence of water at great depths so charged with gas as to effervesce when brought to the surface is not wholly false. This phenomenon is observed when water is brought from great depths in the hot equatorial and tropical regions. Near the bottom the water may have a temperature bordering on the freezing point, and will contain a corresponding amount of air. Brought to the surface, where the temperature may be between 80° and 90° F., it can no longer contain the same amount of gas; and, if in a glass vessel, the walls will be seen to clothe themselves with minute air-cells, somewhat like natural seltzer water which has stood for a little time in an open vessel. At the surface the percentage of oxygen varies between 33 and 35 per cent., the higher number having been observed in a water collected almost on the Antarctic circle; the smallest percentages have been found in the Trade wind districts. In bottom waters the absolute amount is greatest in Antarctic regions, and diminishes generally towards the north. The oxygen percentage is greatest over "diatomaceous oozes," and least over "red clays" with peroxide of manganese, and over "blue muds" it is greater than over "globigerina oozes." In intermediate waters the very remarkable fact was observed that the oxygen diminishes down to a depth of 300 fathoms, where it attains a minimum, after which it rises. The following figures will show the nature of this phenomenon:

Depth (fathoms)....	0	25	50	100	200	300	400	800	Between 800 and bottom.
Oxygen (N+O=100)	33·7	33·4	32·3	30·2	25·4	11·4	15·5	22·6	23·5

It is evident from these figures that between 200 and 400 fathoms there is a great consumption of oxygen going on, and, as it is difficult to conceive its being consumed otherwise than by living creatures, the conclusion is forced on us that animal life must be particularly abundant and active at this depth, or at least more abundant than at greater depths; for at less depths there is more opportunity of renewal of the oxygen, by reason both of the greater proximity to the surface and of the existence of vegetable life. This conclusion was borne out by the numerous experiments made by Mr. Murray with the tow-net at intermediate depths, which went to prove the existence of abundance of animal life down to 400 fathoms, vegetable life never extending to much below 100 fathoms. Below 400 fathoms life is sparingly met with.

THE CHEMICAL COMPOSITION OF THE FLESH OF THE HALIBUT.

THE halibut (*Hippoglossus americanus*, Gill), which abounds in the waters of the Atlantic, as far north as Newfoundland and as far south as Cape Hatteras, is highly valued as food in the United States. The flesh is of a fine white color, is delicate and tender, and resembles that of the whiting (*Merlangus vulgaris*). The fresh flesh of the halibut has been analyzed by Chittenden, and he gives in his paper the results of his examination side by side with those of Payen, who investigated the composition of the flesh of the whiting. They are as follows:

	Halibut.	Whiting.
Water	82·87	82·95
Solid constituents	17·12	17·05
Ash	1·08	1·08
Fatty matter	1·36	0·38
Nitrogen	2·01	2·41

The flesh of the American fish, when dried at 100° C., was found to consist of

Carbon	50·38
Hydrogen	7·43
Nitrogen	11·68
Oxygen	6·35
Ash	24·16
	100·00

and of the above constituents 7·11 per cent. were present in the form of fatty matter. The ash of the flesh of this fish is composed of

Silicic acid	0·32
Chlorine	11·11
Carbonic acid	1·13
Sulphuric acid	1·30
Phosphoric acid	34·36
Iron	0·19
Lime	0·15
Magnesia	2·43
Potash	37·07
Soda	12·22
Lithia	trace.
	100·28

Some considerable part of the alkaline metals which the author gives as oxides in the above percentage numbers must in reality be present in the form of *chlorides*, combined with the 11·11 per cent. of chlorine, and the total constituents found must therefore fall short of the total above given. In fact, the 11·11 per cent. of chlorine has, so to speak, been left out in the cold, uncombined, and must take the place of oxygen in these calculated results; this will reduce the total considerably. More than 70 per cent. of the constituents of the ash, according to the author's mode of regarding them, consists of phosphoric acid and potash.—*American Journal of Science.*

THIRTY-ONE pounds of iron have been made into wire upwards of 111 miles in length, and so fine was the fabric that it resembled horse-hair.

ABSORPTION OF ANTIMONY AND ARSENIC BY CHARCOAL.

By WILLIAM SKEY,
Analyst to the Geological Survey of New Zealand.

SOME time back I showed that charcoal, when freshly made or ignited, absorbs from their aqueous or acid solutions, several substances not before known as being affected in this manner, and I proposed to apply this reaction to the purification of certain of our chemical reagents from substances difficult or tedious to remove by the processes now in use for this purpose.

Since then I have made further investigations in this direction, and find that antinomy and arsenic can be so largely removed from solutions of their oxides or chlorides in moderately strong hydrochloric acid (with a little tartaric acid in the case of antimony) by fresh charcoal, that neither of them can be detected therein by Reinsch's test, although before such process was applied both were abundantly evidenced to the test named.

Thus commercial sulphuric and hydrochloric acids diluted with a little water can be purified from either of these substances by agitating them intermittingly for a short time with fresh charcoal, and then filtering off; application of heat to the mixture expedites this result.

The charcoal used does not appear to give up any portion of either the antimony or arsenic when digested with aqueous solution of potash, hence I consider it very probable that it would absorb either of these metals from alkaline solutions also. Such charcoal, however, when placed in voltaic contact with pure zinc in hydrochloric acid, evolves antimoniuretted or arseniuretted hydrogen (as the case may be) very perceptibly, and it can be wholly divested of either of these substances, when treated in this manner.

In connection with this evolution of such hydrides from charcoal under the circumstances just stated, I will observe here that sulphur, when absorbed by charcoal, is, as I have already shown, also given off, and as a hydride, when the charcoal containing it is connected voltaically with zinc in suitable acids, whereas hot aqueous solutions of potash do not seem to dissolve this sulphur. It appears, therefore, that the character of the absorption of sulphur by charcoal is the same as that of the absorption of antimony and arsenic by this substance.

In examining for minute quantities of either antimony or arsenic by Reinsch's or Marsh's test, I would recommend that the acids used for this (even though purporting to be free from these metals) be filtered through fresh charcoal just before using them, as they frequently extract small quantities of these impurities from the bottles in which they are stored.

RUSSIAN CHEMICAL SOCIETY, ST. PETERSBURG.

May, 1877.

W. ALEXEYEFF, "*On the Reciprocal Solubility of Phenol and Water and Amylic Alcohol and Water.*" The experiments were conducted by sealing weighed amounts of the substances in a glass tube and ascertaining at what temperature they united to form a clear solution. The solubility of phenol in water increases at first in direct ratio with the temperature, then more rapidly, until at 84° it is soluble in all proportions. As no alteration in the increase of solubility takes place at the fusing point of phenol, it is considered that a change in the state of aggregation of a substance does not affect the solubility. The solubility of amyl alcohol in water decreases until a minimum at 50° is reached, and then ascends gradually. These facts are regarded as being in consonance with the hypothesis that an increase of the solubility with the temperature takes place only in the case of such compounds as form either a stable hydrate or no hydrate.

G. GUSTAVSON, "*New Method of Introducing Bromine into the Aromatic Hydrocarbons in the Presence of Aluminium Bromine.*" The author finds that by the slow addition of the aromatic hydrocarbons to an excess of bromine containing a small amount of Al_2Br_6, the complete replacement of the hydrogen atoms in the benzene skeleton—although not in the side-links—is caused. The reaction is exceedingly violent, and must be performed in a cooling mixture. Benzene yields by this method, at 0°, C_6Br_6 quantitatively; toluen gives $C_7Br_8H_2$; and mesitylen is changed into the tribromomesitylen of Fittig and Storer. By reversing the process, adding a limited quantity of bromine to a solution of Al_2Br_6 in a hydrocarbon, any desired number of hydrogen atoms can be substituted. The method is a vast improvement on that hitherto used, of exposing the hydrocarbons to the action of Br and I at 400° for a length of time.

A. ELTEKOFF, "*Structure of the Amylen Boiling at 25°.*" The experiments of Wischnegradsky, showing this to be a mixture of two amylens, have been confirmed by changing them into bromides, and submitting the latter to the action of alcoholic potash. The results are valenylen boiling at 35°, and ethyl-valeryl-oxide, the latter of which could only be formed from the unsymmetrical melhyl-ethyl-ethylen.

I. SETSCHENOFF, "*Absorption of CO_2 by Blood.*" The chief results of the author's investigations are the following: 1. The serum of blood possesses a coefficient solubility for CO_2, not far removed from that of water, and can be considered, in that regard, as a weak solution of phosphate or carbonate of soda. The quantities of CO_2 absorbed are, however, much dependent on temperature and pressure. 2. The alkalies decomposed by the CO_2 are albuminates containing paraglobuline. 3. The red blood cells possess a much greater coefficient of solubility than water. It is, however, much more affected by variations of temperature and pressure than that of serum, and these variations affect inversely the amounts of merely dissolved and of chemically combined CO_2. 4. CO_2 is found in all cases in both serum and cells, the normal saturation in the veins corresponding to the absorption at 37° and 50 m.m. pressure.

CHEMICAL NOTES.

DETERMINATION OF SUGAR.—M. R. Sacchse.—The author has tried the method recommended by Knab (tome xiv, p. 215), founded on the reduction of mercury cyanide in an alkaline solution by means of glucose. As an indicator to mark the end of the reaction, he makes use of an alkaline solution of stannous oxide, which, in a drop of the solution of mercury cyanide placed upon a white porcelain slab, gives a black spot more or less inclining to brown, according to the proportion of mercury. When all the mercury has been reduced by the sugar, a spot is no longer produced. The indication is very exact, but the results of the method are not constant. The author finds that an alkaline solution of mercuric iodide may advantageously be substituted for

the mercuric cyanide. 4·4 grms. of pure mercury iodide are dissolved in an aqueous solution of 25 grms. of potassium iodide, 80 grms. of caustic potasse are added, and the solution is diluted in 1 litre. To make use of the solution 40 c.c. are placed in a capsule, brought to a boil, and the saccharine liquid is introduced by means of a burette. Experiments made with pure glucose shows that 40 c.c. of the mercuric liquid (0·72 grm. HgI_2) corresponds on an average to 0·501 grm. glucose. In other words $2HgI_2 = C_6H_{12}O_6$. Inverted sugar reacts upon the mercuric iodide in other proportions than glucose (dextrose), as 40 c.c. correspond to 1·1 82 grm. of inverted sugar.

ON VAPOR DENSITIES.—M. H. Sainte-Claire Deville.—The author has shown in a former communication that the volumes of oxygen, chlorine, hydrochloric acid, and hydrochlorate of ammonia, whose weights are equivalent to 39 grms. of potassium, are among themselves respectively as 1, 2, 4, and 4. It is easily shown that simple bodies represent 1 or 2 volumes of vapor; the the binary compounds represent 2 or 4 volumes; and that, lastly, we only find 8 volumes in very complex bodies, in particular in salts with volatile bases and acids. The author has never been able to understand (and many atomists are of the same opinion) why, without strict proof, an attempt has been made to erase from the number of bodies which may exist in the state of vapor those which represent 8 volumes. He gives in the present paper a summary of the arguments against this attempt.

OXIDES OF IRON.—M. H. Moisson.—If ferric oxide obtained by calcining oxalate of iron is heated to 350° in a current of pure hydrogen for thirty minutes the result is a black magnetic powder, not pyrophoric, and having the composition of magnetic oxide. It is not a mixture of ferric oxide and metallic iron. If ferric oxide is kept for twenty minutes in a current of pure dry hydrogen at 500°, we obtain a black powder duller than the former, magnetic, pyrophoric, and having the composition and properties of ferrous acid. At 500° it decomposes carbonic acid, and becomes transformed into magnetic oxide. If ferric acid is reduced by hydrogen at 700° we obtain metallic iron which is not pyrophoric. At 1000° the pyrophoric ferrous oxide obtained at 440° loses its properties. There are, therefore, two allotropic ferrous oxides.

USE OF ELECTRO-MAGNETIC MACHINES WITH CONTINUOUS CURRENTS.—The author infers from his experiments that in electro-metallurgical operations it is more economical to arrange the baths for tension than for quantity. M. Jamin, on presenting M. Gramme's paper, remarked that M. A. Thénard had previously communicated experiments proving that the total amount of copper deposited by a Gramme machine is increased considerably by directing the current through a series of baths, whilst the sum of the deposits remains sensibly constant when the baths are arranged not for tension, but for quantity.

INFLUENCE OF A MECHANICAL ACTION ON THE PRODUCTION OF VARIOUS HYDRATES IN SUPERSATURATED SOLUTIONS.—M. D. Gernez.—The author finds that if a rigid rod is introduced into a supersaturated solution, or rubbed against the sides of the vessel below the level of liquid, the formation of crystals may be determined. M. Gernez specifies three cases; crystals of the least hydrated salt may be produced, as when clear and very concentrated solutions of the sulphate of soda below 8° are thus treated. Secondly, there are produced crystals of the most hydrated salt in solutions where, nevertheless, the other hydrate may be formed by the introduction of a crystalline germ. This takes places with the acetate of soda. Or thirdly, either hydrate may be produced according to the intensity of the mechanical action. This case has been observed in concentrated solutions of chloride of calcium.

EOSIN.—B. Baeyer has given certain additional particulars as to the manufacture of eosin. Fluorescein is obtained by heating 5 parts of anhydrous phthalic acid to 200° along with 7 parts of resorcin. The mass swells up and solidifies in the course of three to six hours. Fluorescein is extracted from this crude product by boiling with alcohol. It is a feeble acid, and dyes silk and wool a fast yellow with a reddish cast. For the preparation of eosin the fluorescein is suspended in four parts of glacial acetic acid; and solution of bromine in glacial acetic acid, containing 20 per cent. of the former, is added. Tetrabrom-fluorscein (eosin) separates out in red crystals.—*Reimann's Färber Zeitung.*

DETERMINATION OF PIPERINE IN PEPPERS.—MM. P. Cazeneuve and O. Calloil.—The ground pepper is treated with twice its weight of slaked lime in a large quantity of water. The whole is heated to a boil for fifteen minutes. The mixture is then dried in a water bath, and the pipero-calcareous powder is placed in the author's continuous digesto-distillatory apparatus and exhausted with commercial ether. The liquid is partially distilled off, and the residue is abandoned to spontaneous evaporation, when the piperin is obtained in voluminous crystals tinged slightly yellow by a trace of resinous matter. By recrystallization from boiling alcohol it may be obtained in prismatic crystals of a very pale yellowish tint—its normal color. White Singapore pepper contains 9·15 of piperine; that of Sumatra ranges from 7·15 to 8·10 per cent., whilst Penang pepper falls as low as 5·24.

MANUFACTURE OF CHLORINE BY DEACON'S PROCESS.—A. C. Jurisch.—The author examines the causes which interfere with or hinder the production of chlorine in Deacon's process. Like M. Hasenclever he arrives at the result that the perturbations observed are due to sulphuric acid, but whilst Hasenclever ascribes this influence to a chemical action the author pronounces it chiefly mechanical, the balls of clay becoming coated with an inert layer. He points out that the proportion of hydrochloric acid decomposed depends on three factors: (1.) The proportions of the mixture of air and hydrochloric acid gas. (2.) The rapidity of the gaseous current. (3.) The temperature of the gaseous mixture and of the balls of clay. The proportion of hydrochloric acid in the gaseous mixture varies from 20 to 60 per cent. Mr. Hurter has found that, other conditions being equal, the relative quantity of hydrochloric acid decomposed diminishes with the proportion of air, and increases with the dilution of the hydrochloric acid. The quantity of hydrochloric acid decomposed is the greater the slower is the gaseous current. The decomposition of hydrochloric acid augments with the rise of temperature.—*Dingler's Polytechnisches Journal.*

NEW FORMATION OF ROSANILINE.—Dale and Schorlemmer produce rosaniline by heating red aurin to 150° for several days along with ammonia. The details of the process are given in the *Berichte der Deutschen Chemischen Gesellschaft*, 1877, 10.

AN aqueous solution of eosin is recommended as a red ink in *Reimann's Färber Zeitung.*

[NATURE.]

ON DROPS.

AMONG the many ways in which electricity is called in to give assistance in various physical investigations, one of the most elegant and interesting is the application of the electric spark to render momentarily visible a body that is rapidly moving or changing its form. The duration of the electric spark is so short—probably not more than $\frac{1}{1000000}$ of a second—that a body, such as a rotating wheel or oscillating rod, moving in a dark room with extreme rapidity, will, if illumined by an electric spark, seem stationary, since the wheel or rod has not time to change its position appreciably during the short instant for which it is visible. If the spark be bright, the impression is left on the eye long enough for the attention to be directed to it, and for a clear idea to be formed of what has been seen.

The writer of this article has recently applied this method to watching the changes of form in drops of various liquids falling vertically on a horizontal plate. As usually seen, a drop of water falling from a height of ten or twelve inches on a smooth solid substance, such as glass or wood, seems to make an indiscriminate splash. The whole splash takes place so quickly that the eye cannot follow the changes of form; the impression made by the last part of the splash succeeding that of the first part so quickly as to confuse it.

A little careful observation, however, shows that the drop passes through very definite symmetrical forms, and that a splash is by no means an irregular hap-hazard phenomenon.

Let the reader let fall a few drops of milk, about ¼ inch in diameter, on a smooth dark surface of wood or paper, from a height of, say, six inches (milk is better than water, as it is easier to see, especially on a dark ground); he will observe that the liquid makes a blot with a more or less regular undulated edge, but the splash is too quick to follow with the eye.

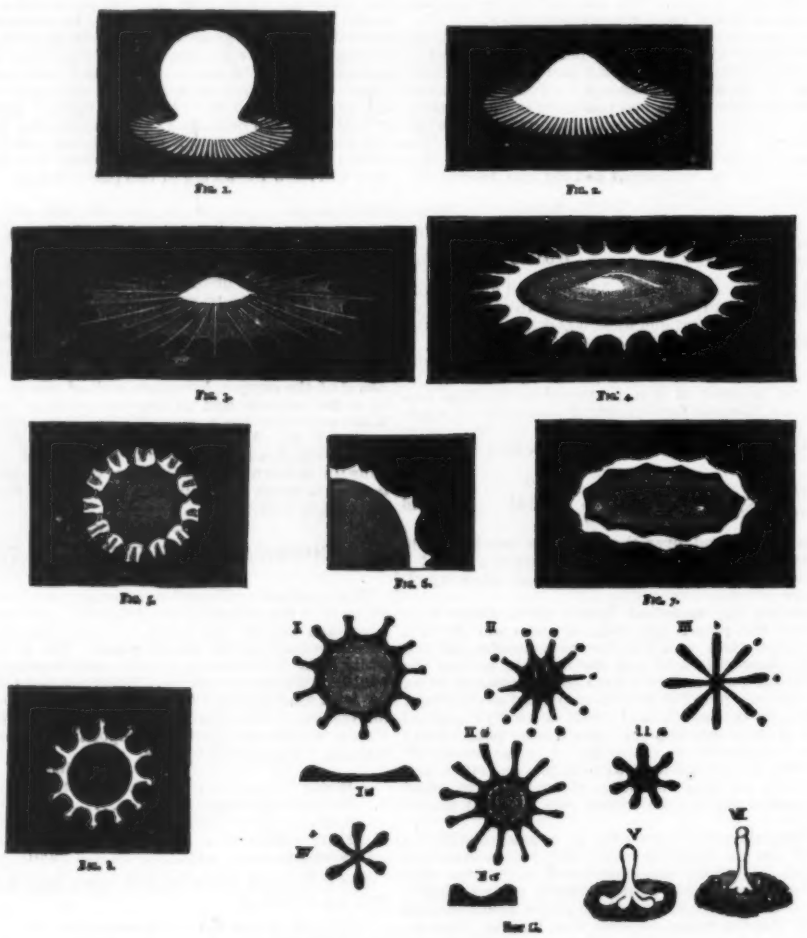
The glimpse that may be caught of the drop in the way described is obtained when the drop is really almost stationary, having flattened itself out on the plate and being on the point of contracting again to its original form.

That a drop if so flattened out will recover itself is seen on pressing down a drop of mercury with the finger or a drop of water with a piece of black-lead or other substance to which it does not adhere. On removing the pressure the drop springs back to its old form; the force which causes this being exerted by the curved surface of the liquid at the edge of the flattened drop, on the liquid within. The flatter the drop becomes the greater is the curvature of the edge and the greater the corresponding pressure tending to restore it to its original globular form. The extent to which a drop that has fallen on a plate will spread out depends on the velocity with which it strikes the plate, *i.e.*, on the height of fall; so that as long as the drop returns to the globular form the phenomenon of the splash may be regarded as an oscillation similar to that of a pendulum; the velocity of the liquid outwards being checked, overcome, and finally reversed by the ever-increasing pressure of the curved edge, just as a pendulum has its velocity checked, overcome, and finally reversed by the action of gravity.

It is only when the height of fall is very great that the liquid flies off in all directions and the splash ceases to be an oscillation; this case corresponds to that of a simple pendulum started with a blow so violent as to break the string.

But the liquid star and the complicated pattern on the smoked glass show that the splash is not a simple spreading out of the drop equally in all directions to return again.

In order to observe the form of the drop at any given instant during the splash, it is necessary to make use of the electric spark and to take advantage of the fact that drops of the same size falling from the same height will all behave in the same way.



WORTHINGTON'S ILLUSTRATIONS OF DROPS.

Let him now substitute a drop of mercury for the milk. By watching the splash very intently he will be able to catch a glimpse of the mercury spread out in the symmetrical star-like form of Fig. 11*a* of Set 2. After the drop has been thus spread out it recovers its globular form, since the mercury does not wet the plate. On increasing the height of fall a few inches, it will be noticed that small drops split off in a more or less complete circle, and are left lying on the plate, while the rest of the drop gathers itself together in the middle of the circle.

The chief reasons why these appearances could not be seen with milk is that the milk wets the glass or wood and sticks to it, while the mercury does not. But by smoking a slip of glass or card tolerably thickly in the flame of a candle we get a finely-divided surface of lamp-black, to which the milk does not adhere any more than the mercury, and by very careful watching we may notice that the same radial star is formed by the milk, but it is much more difficult to catch sight of than the mercury star. But if the mark on the lamp-black be examined after the drop of milk or mercury has rolled away it will be found to consist of delicate concentric rings with numberless fine radial striæ where the smoke has been swept away. These may be seen very well by holding the glass plate up to the light if it has not been too thickly smoked.

The marks thus made are very beautiful and symmetrical, and it will be found, if the glass be uniformly smoked, that the same-sized drops of the same liquid falling from the same height will produce almost exactly similar marks: while if the height be changed the mark on the lamp-black will be somewhat changed; and it is a fair inference, if each drop makes almost exactly the same complicated, symmetrical mark, that the splash of each drop takes place in almost exactly the same way.

It will be necessary to let a drop, say of mercury, fall on a plate in comparative darkness, and to produce a strong spark at the instant the bottom of the drop comes in contact with the plate, and so illumine it; the observer will then see the drop in the form it has at that instant.

A second drop must be let fall in the same way, and be illumined by the spark not at the first moment of contact, but a shade later, say $\frac{1}{10000}$ second later, when the drop will have spread itself out slightly on the plate, and similarly we must illuminate a third drop a shade later than the second, and so on. The observer can, after a little practice, draw from memory on each occasion the drop in the form in which he has seen it. It will be seen that the process consists in isolating consecutive phases of splash from those that precede and follow, and which take place in the darkness, and so do not confuse what has been seen as they would do in continuous daylight.

The device adopted by the writer for so timing the appearance of the spark as to illumine the drop at any desired phase of the splash consisted essentially in breaking the current of an electro-magnet at the instant the drop began to fall; the magnet thus ceasing to act, releases a spring which immediately begins to pull the terminal wire of a strong electric current out of the other terminal, which is a cup of mercury, and the strength of the spring and the depth of immersion of the wire in the mercury are so adjusted that the wire leaves the surface of the mercury, and the required spark is produced at the instant the drop reaches the plate.

For the next drop the spark is made to appear a shade later, either by slackening the spring or increasing the depth to which the terminal wire is immersed in the mercury.

The above figures have been drawn in the way described, and show the behavior of a drop of mercury about ¼ in. in diameter, falling from a height of about three inches

on to a glass plate. Each figure represents a rather later stage of the splash than the preceding.

Set 2 was drawn from the final stages of a milk drop, ¼ in. in diameter, falling 4 in. on to smoked glass; but the forms are almost identical with those of mercury. Of this set Ia and IIa′ are vertical central sections of the middle part of the drop, while IIa and IIIa are alternative forms of II and III.

From the ends of the rays of Fig. 4, usually twenty-four in number, small drops often split off. These are not shown in the figure. One of the most curious features of the phenomenon is the transition from twenty-four rays to twelve arms, shown in Fig. 5. The beauty of many of the forms, especially of the ridged shell-like form shown in Fig. 4, when composed of shining quicksilver apparently rigidly fixed, is very striking. Very similar forms are obtained with milk, but, whether with milk or mercury, are liable to occasional variations. For a more detailed account the reader is referred to the "Proceedings of the Royal Society," Nos. 174 and 177, 1876-77. A. M. WORTHINGTON.

THE REMARKABLE SILVER AND COAL MINES OF PERU.

IN our SUPPLEMENT, Nos. 83 and 84, we gave a description of the wonderful railway works, lately built over the Andes to the Peruvian mining regions. We now present a description of these mining localities, which we find in the *Mining Journal*, written by Henry Sewell, M.E., F.R.S.

After an absence of 20 years from the country, I found great changes in Lima. The streets well paved, and as good sewage as we have in London. Magnificent houses replacing the old Spanish edifices. Callao, the principal seaport, completely transformed, having a magnificent dock. Two railways connect Lima with this port at present. Mr. Henry Meiggs had completed the opposition line, so that the summit of the Andes at an elevation of 15,000 ft. is now in direct communication with the sea. This is a wonderful piece of engineering, and will place one of the most celebrated silver mining districts of the world in direct steam communication with Europe—the celebrated Cerro de Pasco, of which Alexander von Humboldt in his works speaks with enthusiasm, the quantity of silver produced was something fabulous. The district will before long be tapped by a great tunnel which has been undertaken by Mr. Henry Meiggs. A contract was celebrated not long since between all the mine owners of the district and Mr. Meiggs, the Government guaranteeing its fulfillment through various steps taken for that purpose.

I left Lima in the month of November, 1875, and shipped myself in one of the many steamers of the Pacific Steam Navigation Company, with a course due north, in order to land at the port of Casma, this being the starting point for crossing the cordillera to the inland town of Huaras, Province of Ancachs. This city will before many years be placed in direct steam communication with the port of Chimpote; this railway is also under contract with Mr. Henry Meiggs, and is being proceeded with gradually. We landed at the Port of Casma, and rode up the river to the small inland town of the same name, some 10 miles distant, with the object of making preparations there, and procuring mules and the necessary equipment for crossing the first or coast range, as it is called. Here the night was passed. Early next morning at 5 A.M. we went to the Chinese restaurant and were served with good coffee, etc., starting at 5.30. It is curious that all over Peru we have industrious Chinamen who minister to all the wan s of travelers. Nearly all these are men who have completed their four or five years' labor contract. We continued traveling till 12 o'clock, for the greater part of the time up the river of Casma, and stopped at the first village, called Juatann, at an elevation of 2,700 feet. Here the only civilized beings who could supply us with food were Chinamen, and they procured us a place to sleep in. Like the River Rimac, the Casma is also encased in between high mountains, perfectly destitute of trees and vegetation; in fact, the coast of Peru and its mountains are completely barren, but we have every 60 to 100 miles streams flowing from the Andes to the coast; in fact, every now and then a transverse oasis from east to west. On these rivers we meet with magnificent sugar and cotton estates, but as we travel towards the first range these valleys narrow down to nothing, and on reaching the summit the waters divide. We rose, as usual, very early, as it is necessary to avoid the great heat of the sun, and rest during the middle of the day for some hours, and arrived at the village of Pariacoto at an elevation of 4,7 0 ft. Next day it was necessary to push on and reach the only farm left on the river, and arrived at 7 P.M. at Chacchan, the elevation 7,600 ft., where we were most hospitably treated by the owner, a Frenchman, who fattens cattle, these being sold for the Lima market. We rose at 4 o'clock the next morning, having the hardest day's work to overcome, that of crossing the first ridge of the Andes, Sierra Negra, or snowless coast range, at an elevation of 15,700 feet. Already, at 13,000 ft., we commenced feeling the effects of the rarification of the air; this is a species of sea sickness with a disagreeable pain in the forehead. On reaching the summit, or divide, the greatest sight the mind can picture to itself is brought before the traveler. One of the most sublime panoramas the world can show presents itself across the Valley of Huaras. Below at about 5,000 ft. the beautiful and cultivated Valley of Huaras is seen, with its picturesque city, numbering some 12,0 0 inhabitants. Above it a panorama and ocean of snow in the distance, rising to 18,000 ft. above the level of the sea, and it is an imposing spectacle. The mind comes to a stand still, and what the traveler does is to gaze and gaze at the mighty expanse of snow. Looking perpendicularly at the snow-clad peaks, they appear like so many ladders up to heaven, and to the north and south, as far as eyesight can reach, endless snow.

Feeling the sickness and pain in the forehead increasing we had to tear ourselves away from this grand sight. We soon commenced the descent of 5,000 ft, and reached the city of Huaras about 5 o'clock in the afternoon. It is situated between the two, or rather parallel, ranges of the Andes. The valley is not more than five miles wide; the height of this city above the sea is 10,400 ft. In order to get accustomed to the rarefied air we remained here some days, preparing thus for the greater journey across the true Andes range, which I was aware would take us to an elevation of 17,200 ft. before reaching the silver mineral district of Huallanca. Here we were feasted by the hospitable people of the city of Huaras with dinners, balls, etc. In return I assisted them to erect their splendid fountain which they had received from Europe for their principal square, and got my English mason to set it up, as their native masons had never had up to that period to contend with a difficulty where ingenuity was required. We left Huaras about the beginning of December, and traveled up the river some 15 miles to the great silver-lead mining district of Recuay. This is situated 14,600 ft. above the sea, and consists of numerous fissure veins in porphyry of argentiferous galenas, containing about 200 ozs. silver to the ton. There being no fuel or timber of any class at this height I recommended the concentration machinery I brought out to Peru to be placed near these mines, where there is any amount of water power, raising the galenas to 75 per cent. lead, and the silver to about 700 ozs. per ton.

On the 8th December we commenced our journey, and on that same day we came to the foot of the snowy Andes, and camped at the hut of an Indian; the elevation here was 15,200 ft. It was very cold, and with difficulty I made a fire of the only fuel to be had in this icy region, called pumapampa—the dried dung of animals. I managed to make some coffee, and warm up my men, who, although half my age, were played out physically and morally. One had been a Prussian volunteer, and fought in several battles in the late Franco-Prussian war. He had been wounded, slept on the snow, and every class of hardship, but the traveling in the heart of the Andes of Peru had disheartened him; it was too much for him. A soldier could not endure the class of hardship that the miner has to go through. I got up at about 1 o'clock, and to my astonishment found 6 in. of snow on the ground. I went to bed again, and rose at 5 o'clock and found some 12 in. of it. I held a council of war with the guide and muleteer, and informed them it would be a most dangerous affair to cross the heart of the Andes with a snowstorm on hand, and that I would delay the journey. He would not hear of it, and not wishing to be considered faint-hearted I at last told him I would follow him on one condition—that if we were lost in the snow through his folly he would be severely punished. We commenced climbing with great difficulty, the mules slipping and snorting, not finding a safe footing, the narrow mule track being covered with snow; we lost our road several times I must say I felt scary, never expecting to get out of it safe this time. But after four hours riding the snowstorm cleared away, but then we had a new foe to face—the fearful glare of the sun on the snow. I had fortunately with me three pairs of green spectacles for self and men, but the poor muleteer had none, and became blind in less than three hours: we had to leave him behind at the hut of an Indian alpaca hunter. We crossed three parallel ranges that day; one was 16,800 ft., and the others about 17,200 ft.; these ranges are called Guarapasca and Chanasuya.

We commenced the descent of the eastern slope of the Andes. It was a most dangerous road all the way; slippery with the snow and mud—in fact, our mules could not walk, but they slid the greater part of the road. At 8 P.M., after fording the river several times, which was accompanied with

The black lines are the coal seams; the last one, to the right of the house, is the argentiferous ores.

some danger on account of the excessive melting of the snow and consequent flooding we reached the mineral caves of Huallanca; these are situated at 14,700 ft. above the level of the sea. The inspection of these mines was a very trying affair, the rarefication of the air causing an extraordinary increase in the pulsation of the heart, and making it dangerous to move about except with great care.

EXTRAORDINARY GEOLOGICAL FORMATION.

These silver mines are situated in the heart of a coal formation, which has been upheaved by the outburst of porphyry, forming immense backbones. The stratification is standing on its beam ends, and almost perpendicular. At a distance of 100 yards from the houses to the right is the adit out of which the argentiferous copper ores are extracted. The class is a new tetrahedrite, containing about 800 ozs. to the ton, and about 150 yards to the left is the first coal seam from where the smith orders his coal to be extracted. The ores containing 100 to 130 ozs. to the ton are thrown over the dumps, their carriage to the coast being too expensive. I recommended the owners to collect all these poorer ores and smelt them in a reverberatory furnace into regulus, thus raising them to about 600 ozs. to the ton. The ore is found in the shaly portion of the formation, as well as in the sandstone. In the latter it is found in a most singular condition—in huge "vughs" or caves, many of these being as much as 25 feet to 30 feet long, and about an equal size in depth. These caves are coated from 2 inches to 3 inches of argentiferous ores, and millions of crystals of tetrahedrite are destroyed by the picks of the miners, who break down the ore in that manner. Some of these caves have produced as much as 30,000 in a single day. The way they are discovered by the native miners is also singular. They follow for months a thin little c eavage of about ¼ of an inch. This contains calcedony, and they drive horizontally and at an incline of 45°, in order not to miss them. They vary in size from a few feet to that above mentioned. I have presented Professor Maskelyne, of the British Museum, with specimens of these ores, as well as coal and the fossils of the formation.

After staying at the town of Huallanca, which is some 4,000 ft. below the mine on a river of that name, and a tributary of the great Maranon, we commenced our return journey to the coast. Our muleteer, having recovered his sight, accompanied us. We took a different route this time, and to the port of Supe, coming down the River Pativilca, which has its source at the foot of a mountain of the same name. I calculated this mountain to be 18,000 ft. high, and with about 3, 0 ft. thickness of congealed snow on its peak, perfectly transparent, and like an immense field of glass.

BITUMEN IN GRANITE.—M. A. Julien records the occurrence, in the granite of the neighborhood of Clermont-Ferrand, of distinct bituminous veins. They are found in a railway cut ing between Royat-les-Bains and Votrie, where they form a sort of network. The bituminous substance is sometimes black and soft. In other places it is a solid, brilliant asphalte, with a resinous lustre, a conchoidal f acture, and a brow, ish or blackish-brown color, and forms veins varying in thickness from two or three lines to two or three centimetres. At the first glance it might be taken for brown flint, but it fuses at the temperature of boiling water, and burns with a clear flame, producing a strong characteristic odor.—*Comptes rendus.*

SILVER MUD IN OREGON.

IN Wasco County, Oregon, there is a flat, thickly studded with springs of a peculiar character, that throw out mud which has overflowed a considerable area. Some months ago it was reported that this mud had been discovered to be argentiferous and very rich, some specimens assaying over $2,000 to the ton. An effort was made to organize a company in this city to work the mud springs, but the enterprise collapsed in consequence of a suspicion raised that the mud had been "salted," and the memorable Arizona diamond field swindle was too fresh in the recollection for the successful prosecution of another fraud in the same line.

Professor Thomas Price analyzed samples submitted, and reported that he had discovered unmistakable evidence of "salting," the microscope showing filings, crystals, and free gold, which he had no hesitation in declaring had been mixed in with the mud with the manifest design of instituting a great swindle.

Professor H. G. Hanks also examined specimens, but while he was inclined to suspect fraud, he was unab'e to determine positively whether the argentiferous mud was an artificial or natural production. Professor Hanks now intimates that the flow of the Oregon mud springs is in reality heavily impregnated with silver, and this result he announced in a paper read before the California State Geological Society on June 5, 1877.

The existence of springs yielding soft mud, charged with free silver, says Professor Hanks, is new to science; and scientific men, both here and at the East, who examined specimens, pronounced them fictitious without hesitation. The specimens latterly examined by Professor Hanks, he says, were very rich, and silver was discovered in a free state. By simple washing the silver could be wholly separated, and when then examined the microscope failed to reveal the source of the precious metal. Had it been filings, a single glance would have sufficed to detect the fact. Had the silver been precipitated from solution by copper it would have been crystallized.

An amalgam of silver and mercury would have yielded a sublimate if strongly heated in a glass tube closed at one end. Such an amalgam introduced into the wet mud, and the whole heated sufficiently to have volatilized the mercury, would have left the substance in a hard, baked state, which could not again have been reduced to the state in which it reached this city. From these conclusions, if the silver had been introduced for fraudulent purposes, the substance was very remarkable, from the fact that some process had been employed not easily understood.

Professor Hanks finally obtained the address of a gentleman represented to him as being of unquestioned character, Richard Hurley, residing in the vicinity of the wonderful springs in Wasco County, and applied to him for information. In reply to Professor Hanks, Mr. Hurley writes:

"There is no mistake as to this mud containing silver. I have assayed over 100 samples which contained silver, some as high as $2,300 to the ton. The samples I obtained from the springs myself. I think the weather has considerable to do with the mud containing silver. I obtain the best results when the weather is warm. Sometimes in one of the larger springs, when the weather is cold, the mud will be of a yellow color, showing no silver; but when the day is warm the mud is blackish-blue, at least in places, and rich in silver. They seem to work more actively in a warm afternoon. Some of them contain a great deal of acid, the bones of animals that fall into them being dissolved in a few months. There are old wells which assay from $5 to $1,200 to the ton.

"One assay I made from the flat, half a mile from any spring, assayed $1,200 to the ton. There is a great deal of salt, almost pure, all over the spring flat. There are between 100 and 200 quartz leads discovered, running in two directions close to the spring. I find silver in several of them, all the way from a few dollars to $100 to the ton. Some of these leads run through the springs—at least, they point in that direction. The altitude of this place is between 4,000 and 5,000 feet."

Professor Hanks also refers in his paper to the recent discovery of a peculiar silver-bearing deposit located in Southwestern Utah. It occurs in the Maud Mine, six miles from Leeds. Some assays as high as $700 per ton have been made. Instead of being sandstone, as supposed, Professor Hanks found the deposit to be sedimentary, but closely resembling the Oregon mud. Under the microscope it has all the appearance of that strange substance. The Oregon mud, if allowed to dry in large quantities, would soon form a similar substance to the Utah mineral, in appearance at least. The silver is in the state of chloride, and is seen under the microscope both amorphous and in crystals. An analysis of the two minerals will be interesting, and may throw some new light on the subject.—*San Francisco Examiner.*

THE ORIGIN OF MINERAL OILS.

AT a recent meeting of the Russian Chemical Society, Mendelejeff presented a report on his examination of the deposits of mineral oil in the Caucasus and in Pennsylvania, and propounded a new and interesting theory as to its probable origin. He questioned the accuracy of the view generally held, of the oil being a product of the decomposition of the fossil remains of organisms. The occurrence of the oil near the earth's surface is to be attributed to the tendency which it possesses to traverse higher and porous sedimentary deposits when exposed to the action of water universally present, and of greater density than itself. The beds of sandstone in which much of the oil is met with do not appear to contain the remains of the organisms to which its origin is ascribed, and the geologist is driven to seek at greater depths the materials which yield it. In the Caucasus the oil occurs in Tertiary beds; in Pennsylvania, in Devonian and Silurian rocks. But, the author maintains, we have no grounds for believing in the occurrence of any extensive deposits of organized structures at a period earlier than the Silurian, and the prevailing view, therefore, can hardly be considered to meet the difficulty. Mendelejeff refers to the theories put forth by Laplace and others respecting the probable mode of formation of our globe, its density as compared with that of the surface layers, so far as we are acquainted

with them, etc., and draws attention to the possibility of the interior of the earth containing metallic masses of vast extent. If it be allowed that the iron is the prevailing metal—and its presence in great quantities in the sun and in meteorites renders his theory a not improbable one—and that metal occurs in combination with carbon, we have the material from which we can conceive the mineral oil to have been derived. Contact with water at high temperatures, and under great pressure, brought about by the upheaval or disruption of any of the overlying sedimentary strata, would result in the formation of metallic oxides and saturated hydrocarbons. The latter, permeating the porous sandstones of higher levels, would condense there, or, after undergoing further change, become the marsh gas of the "gas wells," or be converted into unsaturated hydrocarbons. The invariable association of salt water with mineral oil is not without its bearing on this interesting question. If the view recently advanced by Steenstrup, that the remarkable metallic masses, discovered by Nordenskjöld, in 1870, at Ovifak, Disko Island, Greenland, and generally held to be blocks of meteoric iron, be a correct one, and they are really erupted matter and not of cosmical origin, they narrowly resemble, as regards their composition, which analysis has shown to be to a considerable extent carbide of iron and magnetite, the material which Mendelejeff assumes to be the source of the oil.—*Popular Science Review.*

DIFFUSION OF STRONTIAN IN NATURE.

M. DIEULAFAIT, in a memoir presented to the French Academy of Sciences ("Comptes Rendus," June, 1877), gives the results of his investigations upon this subject, which are of interest in themselves and as bearing upon the general question of the mode of occurrence in nature of comparatively rare substances. His experiments appear to have been carried on chiefly by means of the spectroscope.

M. Dieulafait finds that strontian exists in sea water as carbonate and sulphate. In the former state it may be recognized in 100, in the latter in 4 cubic centimetres of water. The gypsum formed in salt marshes before the deposition of salt contains strontian in such quantity that 1 milligramme of the substance distinctly gives its spectrum; and as in the evaporation of sea water the strontian is always thrown down in the precipitates of carbonate and sulphate of lime which are first produced, it is naturally missing in the sodium chloride and other salts afterwards produced. As strontian is always associated with the carbonate and sulphate of lime dissolved in sea water, it ought also to occur in the hard parts of marine organisms consisting of those salts; and M. Dieulafait has always found it in them with a weight of not more than 1 centigramme. A further investigation showed that this rule applied also to the fossil remains of organisms, for 120 species of Brachiopoda, from the whole series of formations from the Silurian upwards, all furnished the spectrum of strontian with quantities of substance less than 1 centigramme. In the author's opinion all deposits of gypsum, of whatever geological age, have been produced simply by the evaporation of sea water at the ordinary temperature of each epoch; and if this view be correct, and the seas of all times had the same constitution, all specimens of gypsum ought to contain strontian. This was found to be the case in 188 samples of Triassic gypsum, 85 of Tertiary gypsum, 4 of gypsum with mica (so-called metamorphic gypsum), and 6 specimens of the ophitic deposits of the Pyrenees, and the strontian was found to be pretty uniformly diffused throughout the mass of each deposit. In mineral waters also, as might be expected, strontian is at least very generally present in variable quantities.

ON THE PROBABLE ORIGIN AND AGE OF THE SUN.

By JAMES CROLL, LL.D., F.R.S.*

THE total annual amount of radiation from the whole surface of the sun is $8,340 \times 10^{30}$ foot pounds. To maintain the present rate of radiation it would require the combustion of about 1,500 pounds of coal per hour on every square foot of the sun's surface; and were the sun composed of that material it would be all consumed in less than 5,000 years. The opinion that the sun's heat is maintained by combustion cannot be entertained for a single moment. Mr. Lockyer has suggested that the elements of the sun are, owing to its excessive temperature, in a state of dissociation, and some have supposed that this fact might help to explain the duration of the sun's heat. But it must be obvious that, even supposing we were to make the most extravagant estimate of the chemical affinities of these elements, the amount of heat derived from their combination could at most give us only a few thousand years' additional heat. Under every conceivable supposition the combustion theory must be abandoned.

It is now generally held by physicists that the enormous store of heat possessed by the sun could only have been derived from gravitation. For example, a pound of coal falling into the sun from an infinite distance would produce by its concussion more than 6,000 times the amount of heat that would be generated by its combustion. It would, in fact, amount to upward of 65,000,000,000 foot-pounds—an amount of energy sufficient to raise 1,000 tons to a height of 5½ miles.

There are two forms in which the gravitation theory has been presented: 1. The meteoric theory, propounded by Dr. Meyer; and 2. The contraction theory, advocated by Helmholtz. The meteoric theory of the sun's heat has now been pretty generally abandoned for the contraction theory advanced by Helmholtz. Suppose, with Helmholtz, that the sun originally existed as a nebulous mass, filling the entire space presently occupied by the solar system, and extending into space indefinitely beyond the outermost planet. The total amount of work in foot-pounds performed by gravitation in the condensation of this mass to an orb of the sun's present size can be found by means of the following formula given by Helmholtz:

$$\text{Work of condensation} = \frac{3}{5} \cdot \frac{r^2 M^2}{Rm} \cdot g.$$

M is the mass of the sun, m the mass of the earth, R the sun's radius, and r the earth's radius. Taking—

$M = 4,230 \times 10^{27}$ lbs., $m = 11,9,20 \times 10^{21}$ lbs.,
$R = 2,328,500,000$ feet, and $r = 20,889,272$ feet,

we have, then, for the total amount of work performed by gravitation in foot pounds:

$$\frac{3}{5} \cdot \frac{(20,889,272.5)^2 \times (4,230 \times 10^{27})^2}{2,328,500,000 \times 11,920 \times 10^{21}} = 168,790 \times 10^{30} \text{ foot-pounds.}$$

The amount of heat thus produced by gravitation would suffice for 20,237,500 years.

* Of H.M. Geological Survey of Scotland, and author of "Climate and Time."

The conclusions are based upon the assumption that the density of the sun is uniform throughout. But it is highly probable that the sun's density increases towards the centre, in which case the amount of work performed by gravitation would be something more than the above.

At this point, in reference to the age of our globe, geology and physics are generally supposed to come into direct antagonism. For, if it be true, as physicists maintain, that gravitation is the only possible source from which the sun could have derived its store of energy, then the sun could not have maintained our globe at its present temperature for more than about 20,000,000 years. "On the very highest computation which can be admitted," says Professor Tait, "it cannot have supplied the earth, even at the present rate, for more than about 15,000,000 or 20,000,000 years."* The limit to the age of the sun's heat must have limited the age of the habitable globe. All the geological history of the globe would necessarily be comprehended within this period. If the sun derived its heat from the condensation of its mass, then it could not possibly be more than about 20,000,000 years since the beginning of the Laurentian period. But 20,000,000 years would be considered by most geologists to represent only a comparatively small portion of time which must have elapsed since organic life began on our globe.

It is true that the views which formerly prevailed among geologists, in regard to the almost unlimited extent of geological time, have of late undergone very considerable modifications; but there are few geologists, I presume, who would be willing to admit that the above period is sufficient to comprehend the entire history of stratified rocks.

It is the facts of denudation which most forcibly impress the mind with a sense of immense duration, and show most convincingly the great antiquity of the earth.

We know unquestionably that many of the greatest changes undergone by the earth's crust were produced, not by convulsions and cataclysms of Nature, but by those ordinary agencies that we see at work every day around us, such as rain, snow, frost, ice, chemical action, etc. Valleys have not been produced by violent dislocations, nor the hills by upheavals, but both have been carved out of the solid rock by the silent and gentle agency of chemical action, frost, rain, and running water. In short, the rocky face of our globe has been moulded into hill and dale, and ultimately worn down to the sea-level by means of these apparently trifling agents, not merely once or twice, but probably dozens of times over during past ages. Now, when we reflect that with such extreme slowness do these agents perform their work that we might, if we could, watch their operations from year to year and from century to century without being able to perceive that they make any sensible impression, we are necessitated to conclude that geological periods must be enormous. The utter inadequacy of a period of 20,000,000 years for the age of our earth is demonstrable from the enormous thickness of rock which is known to have been removed off certain areas by denudation. I shall now briefly refer to a few of the many facts which might be adduced on this point.

One plain and obvious method of showing the great extent to which the general surface of the country has been lowered by denudation is furnished, as is well known, by the way in which the inequalities of surface produced by faults or dislocations have been effaced. It is quite common to meet with faults where the strata on the one side have been depressed several hundred feet—and in some cases thousands—of feet below that on the other, but we seldom find any indications of such on the surface, the inequalities on the surface having been all removed by denudation. But in order to effect this a mass of rock must have been removed equal in thickness to the extent of the dislocation. The following are a few examples of large faults:

The great Irwell fault, described by Professor Hull,† which stretches from the Mersey west of Stockport to the north of Bolton, has a throw of upward of 3,000 feet.

Some remarkable faults have been found by Professor Ramsay in North Wales. For example, near Snowdon, and about a mile east southeast of Beddgelert, there is a fault with a down-throw of 5,000 feet; and in the Berwyn Hills, between Bryn-mawr and Postgwyn, there is one of 5,000 feet. In the Aran Range there is a great fault, designated the Bala fault, with a down-throw of 7,000 feet. Again, between Aran Mowddwy and Careg Aderyn the displacement of the strata amounts to no less than 10,000 to 11,000 feet.‡ Here we have evidence that a mass of rock, varying from one mile to two miles in vertical thickness, must have been denuded in many places from the surface of the country in North Wales.

The fault which passes along the east side of the Pentlands is estimated to have a throw of upward of 3,000 feet.§ Along the flank of the Grampians a great fault runs from the North Sea at Stonehaven to the estuary of the Clyde, throwing the Old Red Sandstone on end sometimes for a distance of two miles from the line of dislocation. The amount of the displacement, Professor Geikie‖ concludes, must be in some places not less than 5,000 feet, as indicated by the position of occasional outliers of conglomerate on the Highland side of the fault.

The great fault crossing Scotland from near Dunbar to the Ayrshire coast, and which separates the Silurians of the south of Scotland from the old red sandstone and carboniferous tracts of the north, has been found, by Mr. B. N. Peach, of the Geological Survey,¶ to have in some places a throw of fully 15,000 feet. This great dislocation is older than the Carboniferous period, as is shown by the entire absence of any old red sandstone on the south side of the fault, and by the occurrence of the carboniferous limestone and coal measures lying directly on the silurian rocks. We obtain here some idea of the enormous amount of denudation which must have taken place during a comparatively limited geological epoch. So vast a thickness of old red sandstone could not, as Mr. Peach remarks, "have ended originally where the fault now is, but must have swept southward over the lower silurian uplands. Yet these thousands of feet of sandstones, conglomerates, lavas, and tuffs, were so completely removed from the south side of the fault previous to the deposition of the carboniferous limestone series and the coal measures, that not a fragment of them is anywhere to be seen between these latter formations and the old silurian floor." This enormous thickness of nearly three miles of old red sandstone must have been denuded away during the period which intervened between the deposition of the lower old red sandstone and the accumulation of the carboniferous limestone.

Near Tipperary, in the south of Ireland, there is a dislocation of the strata of not less than 4,000 feet,** which brings down the coal measures against the silurian rocks. Here 1,000 feet of old red sandstone, 3,000 feet of carboniferous limestone, and 800 feet of coal measure have been removed by denudation off the silurian rocks. Not only has this immense thickness of beds been carried away, but the silurian itself, on which they rested, has been eaten down in some places into deep valleys several hundreds of feet below the surface on which the old red sandstone rested.

Faults to a similar extent abound on the Continent and in America, but they have not been so minutely examined as in this country. In the valley of Thessolon, to the north of Lake Huron, there is a dislocation of the strata to the extent of 9,000 feet.†

In front of the Chilowee Mountains there is a vertical displacement of the strata of more than 10,000 feet.‡ Professor H. D. Rogers found in the Appalachian coal-fields faults ranging from 5,000 feet to more than 10,000 feet of displacement.

There are other modes than the foregoing, by means of which geologists are enable to measure the thickness of strata which may have been removed in places off the present surface of the country, into the details of which I need not here enter. But I may give a few examples of the enormous extent to which the country, in some places, has been found to have been lowered by denudation.

Prof. Geikie has shown§ that the Pentlands must at one time have been covered with upward of a mile in thickness of carboniferous rocks which have all been removed by denudation.

In the Bristol coal fields, between the river Avon and the Mendips, Prof. Ramsay has shown‖ that about 9,000 feet of carboniferous strata have been removed by denudation from the present surface.

Between Bendrick Rock and Garth Hill, South Glamorganshire, a mass of carboniferous and old red sandstone, of upward of 9,000 feet, has been removed. At the vale of Towy, Carmarthenshire, about 6,000 feet of silurian and 5,000 feet of old red sandstone—in all about 11,000 vertical feet—have been swept away. Between Llandovery and Aberaeron a mass of about 12,000 vertical feet of the silurian series has been removed by denudation. Between Ebwy and the forest of Dean, a distance of upward of twenty miles, a thickness of rock varying from 5,000 to 10,000 feet has been abstracted.

Prof. Hull found¶ on the northern flanks of the Pendle Range, Lancashire, the permian beds resting on the denuded edges of the millstone grit, and these were again observed resting on the upper coal measures south of the Wigan coal-field. Now, from the known thickness of the carboniferous series in this part of Lancashire, he was enabled to calculate approximately the quantity of carboniferous strata which must have been carried away between the period of the millstone grit and the deposition of the permian beds, and found that it actually amounted to no less than 9,900 feet. He also found in the vale of Clitheroe, and at the base of the Pendle Range, that the coal measures, the whole of the millstone grit, the Yoredale series, and part of the carboniferous limestone, amounting in all to nearly 20,000 feet, had been swept away—an amount of denudation which, as Prof. Hull remarks, cannot fail to impress us with some idea of the prodigious lapse of time necessary for its accomplishment.

In the Nova Scotia coal-fields one or two miles in thickness of the strata have been removed in some places.**

It may be observed that, enormous as is the amount of denudation indicated by the foregoing figures, these figures do not represent in most cases the actual thickness of rock removed from the surface. We are necessitated to conclude that a mass of rock equal to the thickness stated must have been removed, but we are in most cases left in uncertainty as to the total thickness which has actually been carried away. In the case of a fault, for example, with a displacement of (say) one mile, where no indication of it is seen at the surface of the ground, we know that on one side of the fault a thickness of rock equal to one mile must have been denuded, but we do not know how much more than that may have been removed. For anything which we know to the contrary, hundreds of feet of rock may have been removed before the dislocation took place, and as many more hundreds after all indications of dislocation had been effaced at the surface.

But it must be observed that the total quantity of rock which has been removed from the *present* surface of the land is evidently small in proportion to the total quantity removed during the past history of our globe. For those thousands and thousands of feet of rock which have been denuded were formed out of the waste of previously existing rocks, just as these had been formed out of the waste of yet older rock masses. In short, as a general rule, the rocks of one epoch have been formed out of those of preceding periods, and go themselves to form those of subsequent epochs.

In many of the cases of enormous denudation to which we have referred, the erosion has been effected during a limited geological epoch. We have, for example, seen that upward of a mile in thickness of carboniferous rock has been denuded in the area of the Pentlands. But the Pentlands themselves, it can be proved, existed as hills, in much their present form, before the carboniferous rocks were laid down over them; and as they are of lower old red sandstone age, and have been formed by denudation, they must consequently have been carved out of the solid rock between the period of the old red sandstone and the beginning of the carboniferous age. This affords us some conception of the immense lapse of time represented by the middle and upper old red sandstone periods.

Again, in the case of the great fault separating the silurians of the south of Scotland from the old red sandstone tracts lying to the north, a thickness of the latter strata of probably more than a mile, as we have seen, must have been removed from the ground to the south of the fault before the commencement of the carboniferous period. And, again, in the case of the Lancashire coal-fields, to which reference has been made, nearly two miles of thickness of strata had been removed in the interval which elapsed between the millstone grit and the permian periods.

As we are enabled, from geological evidence, to form some rough estimates of the extent to which the country in various places has been lowered by subaerial denudation during a given epoch, it is evident that we should have a means of arriving at some idea of the length of that epoch, did we know the probable rate at which the

* "Recent Advances in Physical Science," p. 175.
† "Memoirs of the Geological Survey of Lancashire," 1862.
‡ "Memoirs of the Geological Survey of Great Britain," vol. iii.
§ "Memoir to Sheet 32," Geological Survey-map of Scotland.
‖ *Nature*, vol. xiii., p. 390.
¶ "Explanation to Sheet 15," Geological Survey-map of Scotland.

* Jukes's and Geikie's "Manual of Geology," p. 441.
† "Geology of Canada," 1863, p. 61.
‡ Safford's "Geology of Tennessee," p. 309.
§ Memoir to Sheet 32, "Geological Survey of Scotland."
‖ "Denudation of South Wales;" "Memoirs of Geological Survey," vol. i.
¶ *Quarterly Journal of the Geological Society*, vol. xxiv., p. 338.
** Lyell's "Student's Manual," chap. xxiii.

denudation took place. If we had a means of forming even the roughest estimate of the probable average rate of subaerial denudation during past ages, we should be enabled thereby to assign approximately an inferior limit to the age of the stratified rocks. We could then tell, at least, whether the amount of subaerial denudation known to have been effected during past geological ages could have been accomplished within 20,000,000 years or not, and this is about all with which we are at present concerned. And if it can be proved that a period of 20,000,000 years is much too short to account for the amount of denudation known to have taken place, then it is certain that the gravitation theory cannot explain the origin and source of the sun's heat.

A very simple and obvious method of determining the present mean rate of subaerial denudation was pointed out several years ago,* viz., that the rate of denudation must be equal to the rate at which the materials are carried off the land into the sea. But the rate at which the materials are thus abstracted is measured by the rate at which sediment is carried down by our rivers. Consequently, in order to determine the present rate of subaerial denudation, we have only to ascertain the quantity of sediment annually carried down by the river systems.

Very accurate measurements have been made of the quantity of sediment carried down into the Gulf of Mexico by the river Mississippi, and it is found to amount to 7,474,000,000 cubic feet. The area drained by the river is 1,224,000 square miles. Now, 7,474,000,000 cubic feet removed from 1,224,000 square miles of surface is equal to $\frac{1}{4\frac{1}{6}\frac{1}{6}}$ of a foot off the surface per annum, or one foot in 4,566 years. The specific gravity of the sediment is taken at 1·9, and that of the rock at 2·5; consequently the amount removed is equal to one foot of rock in about 6,000 years. For many reasons there are few rivers better adapted for affording us a fair average of the rate of subaerial denudation than the Mississippi. In reference to the above I may here quote the words of Sir Charles Lyell: "There seems," he says, "no danger of our overrating the mean rate of waste by selecting the Mississippi as our example, for that river drains a country equal to more than half the continent of Europe, extends through twenty degrees of latitude, and therefore through regions enjoying a great variety of climate, and some of its tributaries descend from mountains of great height. The Mississippi is also more likely to afford us a fair test of ordinary denudation, because, unlike the St. Lawrence and its tributaries, there are no great lakes in which the fluviatile sediment is thrown down and arrested on its way to the sea."†

Rough estimates have been made of the sediment carried down by some eight or ten European rivers; and, although those estimates cannot be depended upon as being anything accurate, still they show that it is extremely probable that the European Continent is being denuded at about the same rate as the American.

I think we may safely assume, without the risk of any great error, that the average rate of subaerial denudation during past geological ages did not differ much from the present. The rate at which a country is lower by subaerial denudation is determined‡ not so much by the character of its rocks as by the sedimentary carrying power of its river systems. And this again depends mainly upon the amount of rainfall, the slope of the ground, and the character of the soil and vegetation covering the surface of the country. And, in respect of these, we have no reason to believe that the present is materially different from the past. No doubt the average rainfall during some past epochs might have been greater than at present, but there is just as little reason to doubt that during other epochs it might have been less than now. We may, therefore, conclude that about one foot of rock removed from the general surface of the country in 6,000 years may be regarded as not very far from the average rate of denudation during past ages.

But some of the cases we have given of great denudation refer to comparatively small areas, and others to beds which form anticlinal axes, and which, as is well known, denude more rapidly than either synclinal or horizontal beds. We shall, therefore—to prevent the possibility of over-estimating the length of time necessary to effect the required amount of denudation—assume the rate to have been double the above, or equal to one foot in 3,000 years.

To lower the country one mile by denudation would therefore require, according to the above rate, about 15,000,000 years; but we have seen that a thickness of rock more than equal to that must have been swept away since the carboniferous period. For, even during the carboniferous period itself, more than a mile in thickness of strata in many places was removed. Again, there can be no doubt whatever that the amount of rock removed during the old red sandstone period was much greater than one mile; for we know perfectly well that over large tracts of country nearly a mile in thickness of rock was carried away between the period of the lower old red sandstone and the carboniferous epoch. Further, all geological facts go to show that the time represented by the lower old red sandstone itself must have been enormous.

Now, three miles of rock removed since the commencement of the old red sandstone period (which in all probability is an under-estimate) would give us 45,000,000 years.

Again, going further back, we find the lapse of time represented by the silurian period to be even more striking than that of the old red sandstone. The unconformities in the silurian series indicate that many thousands of feet of these strata were denuded before overlying members of the same great formations were deposited. And again, this immense formation was formed in the ocean by the slow denudation of pre-existing cambrian continents, just as these had been built up out of the ruins of the still prior laurentian land. And even here we do not reach the end of the series, for the very laurentians themselves resulted from the denudation, not of the primary rocks of the globe, but of previously existing sedimentary and probably igneous rocks, of which, perhaps, no recognizable portion now remains.

Few familiar with the facts of geology will consider it too much to assume that the time which had elapsed prior to the old red sandstone was equal to the time which had elapsed since that period. But if we make this assumption, this will give us at least 90,000,000 years as the age of the stratified rocks.

That the foregoing is not an over-estimate of the probable amount of rock removed by subaerial denudation during past geological ages will appear further evident from the following considerations: The mountain ridges of our globe, in most cases, as is well known, have been formed by subaerial denudation; they have been carved out of the solid block. They stand 2,000, 4,000, or 5,000 feet high, as the case may be, simply because 2,000, 4,000, or 5,000 feet of rock have been denuded from the surrounding country. The mountains are high simply because the country has been lowered. But it must be observed that the height which the mountains reach above the surrounding country does not measure the full extent to which the country has been lowered by denudation, because the mountains themselves have also been lowered. The height of the mountains represents merely the extent to which the country has been lowered. In the formation of a mountain by denudation, say 3,000 feet in height, probably more than 6,000 feet of strata may have been removed from the surrounding country. The very fact of a mountain standing above the surrounding country exposes it the more to denudation, and it is certainly not an exaggerated assumption to suppose that, while the general surface of the country was being lowered 6,000 feet by denudation, the mountain itself was at least lowered by 3,000 feet.

The very common existence of mountains 2,000 or 3,000 feet in height, formed by subaerial denudation, proves that at least one mile must have been worn off the general surface of the country. It does not, of course, follow that the general surface ever stood at an elevation of one mile above the sea-level, since denudation would take place as the land gradually rose. We know that the land was once under the sea, for it was there that it was formed. It is built up out of the materials resulting from the carving out into hill and dale, through countless ages, of a previously existing land, just as this latter had resulted from the destruction of a still older land, and so on in like manner back into the unknown past.

It has now been proved, by the foregoing very simple and obvious method, that the age of the earth must be far more than 20,000,000 or 30,000,000 years. This method, it is true, does not enable us to determine with anything like accuracy the actual age of the globe, but it enables us to determine with absolute certainty that it must be far greater than 20,000,000 years. We have not sufficient data to determine how many years have elapsed since life began on the globe, for we do not know the total amount of rock removed by denudation; but we have data perfectly sufficient to show that it began far more than twice 20,000,000 years ago.

But if the present order of thing has been existing for more than 20,000,000 years, then the sun must have been illuminating our globe for that period, and, if so, then there must have been some other source than that of gravitation from which the sun derived its energy, for gravitation, as we have seen, could only have supplied the present rate of radiation for about one-half that period.

It is perfectly true, as has been stated, that the length of time that the sun could, by its radiation, have kept the earth in a state fit for animal and vegetable life, must have been limited by the store of energy in the form of heat which it possessed. But it does not follow as a necessary consequence, as is generally supposed, that this store of energy must have been limited to the amount obtained from gravity in the condensation of the sun's mass. The utmost that any physicist is warranted in affirming is simply that it is impossible for him to *conceive* of any other source. His *inability*, however, to conceive of another source cannot be accepted as a proof that there *is* no other source. But the physical argument that the age of our earth must be limited by the amount of heat which could have been received from gravity is in reality based upon this assumption—that, because no other source can be conceived of, there is no other source.

It is perfectly obvious, then, that this mere negative evidence against the possibility of the age of our habitable globe being more than 20,000,000 or 30,000,000 years is of no weight whatever when pitted against the positive evidence here advanced that its age must be far greater.

Now, in proving that the antiquity of our habitable globe must be far greater than 20,000,000 or 30,000,000 years, we prove that there must have been some other source in addition to gravity from which the sun derived his source of energy; *and this is the point which I have been endeavoring to reach by this somewhat lengthy discussion.*

Are we really under any necessity of assuming that the sun's heat was wholly, or even mainly, derived from the condensation of his mass by gravity? According to Helmholtz's theory of the origin of the sun's heat by condensation, it is assumed that the matter composing the sun, when it existed in space as a nebulous mass, was not originally possessed of temperature, but that the temperature was given to it as the mass became condensed under the force of gravitation. It is supposed that the heat given out was simply the heat of condensation. But it is quite conceivable that the nebulous mass might have been possessed of an original store of heat previous to condensation.

It is quite possible that the very reason why it existed in such a rarefied or gaseous condition was its excessive temperature, and that condensation only began to take place when the mass began to cool down. It seems far more probable that this should have been the case than that the mass existed in so rarefied a condition without temperature. For why should the particles have existed in this separate form when devoid of the repulsive energy of heat, seeing that, in virtue of gravitation, they had such a tendency to approach one another?

It will not do to begin with the assumption of a cold nebulous mass, for, the moment that the mass existed as such, condensation—under the influence of the mutual attraction of its particles—would commence. We must therefore assume either that the mass was created at the moment condensation began, or that, prior to this moment, it existed under some other form. There are few, I think, who would be willing to adopt the former alternative. If we adopt the latter we must then ask the question, "In what condition did this mass exist prior to the commencement of condensation?" The answer to this question would naturally be that it existed in a condition of excessive temperature, the repulsive force of heat preventing the particles approaching one another. In short, the excessive temperature was the very cause of the nebulous condition.

But if the mass was originally in a heated condition, then in condensing it would have to part not only with the heat of condensation, but also with the heat which it originally possessed.

It is therefore evident that if we admit that the nebulous mass was in a state of incandescence prior to condensation, it will really be difficult to fix any limit either to the age of the sun or to the amount of heat which it may have originally possessed. The 20,000,000 years' heat obtained by condensation may in such a case be but a small fraction of the total quantity possessed by the mass.

The question now arises: "By what means could the nebulous mass have become incandescent? From what source could the heat have been obtained?" The dynamical theory of heat affords, as was shown several years ago,* an easy answer to this question. The answer is that *the energy in the form of heat possessed by the mass may have been derived from motion in space.* Two bodies, each one-half the mass of the sun, moving directly toward each other with a velocity of 476 miles per second, would by their concussion generate in a *single moment* 50,000,000 years' heat. For two bodies of that mass, moving with a velocity of 476 miles per second, would possess $4,149 \times 10^{28}$ foot-pounds of kinetic energy, and this converted into heat by the stoppage of their motion would give out an amount of heat which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

Why may not the sun have been composed of two such bodies? And why may not the original store of heat possessed by him have all been derived from the concussion of these two bodies? Two such bodies coming into collision with that velocity would be dissipated into vapor and converted into a nebulous mass by such an inconceivable amount of heat as would thus be generated; and when condensation on cooling took place, a spherical mass like that of the sun would result. It is perfectly true that two such bodies could never attain the required amount of velocity by their mutual gravitation toward each other. But there is no necessity whatever for supposing that their velocities were derived from their mutual attraction alone; they might have been approaching each other with the required velocity wholly independent of gravitation.

We know nothing whatever regarding the absolute motion of bodies in space; and, beyond the limited sphere of our observation, we know nothing even of their relative motions. There may be bodies moving in relation to our system with inconceivably velocity. For anything that we know to the contrary, were one of these bodies to strike our earth the shock might be sufficient to generate an amount of heat that would dissipate the earth into vapor, though the striking body might not be heavier than a cannon-ball. There is, however, nothing very extraordinary in the velocity which we have found would be required to generate the 50,000,000 years' heat in the case of the two supposed bodies. A comet having an orbit extending to the path of the planet Neptune, approaching so near the sun as to almost graze his surface in passing, would have a velocity of about 390 miles per second, which is within 86 miles of that required.

It must be borne in mind, however, that the 476 miles per second is the velocity at the moment of collision; but more than one-half of this would be derived from the mutual attraction of the two bodies in their approach to each other. Suppose, for simplicity of calculation, each body to be equal in volume to the sun, and of course one-half the density, the amount of velocity which they would acquire by their mutual attraction would be 274 miles per second. Consequently we have to assume an original or projected velocity of only 202 miles per second. And if the original velocity was 676 per second, the total amount of heat generated would suffice for 200,000,000 years at the present rate of radiation.

On former occasions† I expressed it as my opinion that the total quantity of heat possessed by the sun could not probably exceed 100,000,000 years' heat. But if we admit that the heat was derived from motion in space, there really does not seem any reason why it may not be double that amount.

It will be asked, "Where did the two bodies get their motion?" It may as well, however, be asked, "Where did they get their existence?" It is just as easy to conceive that they always existed in motion as to conceive that they always existed at rest. In fact, this is the only way in which energy can remain in a body without dissipation in space. Under other forms a certain amount of the energy is constantly being transformed into heat which never can be retransformed back again, but is dissipated into space as radiant heat. But a body moving in void stellar space will, unless a collision takes place, retain its energy in the form of motion untransformed for ever.

It will perhaps be urged as an objection that we have no experience of bodies moving in space with velocities approaching to anything like 400 or 600 miles per second. A little consideration will, however, show that this is an objection which can hardly be admitted, as we are not in a position to be able to perceive bodies moving with such velocities. No body moving at the rate of 400 miles per second could remain as a member of our solar system. Beyond our system, the only bodies visible to us are the nebulæ and fixed stars, and they are visible because they are luminous. But the fixed stars are beyond doubt suns similar to our own; and, if we assume that the energy in the form of heat and light possessed by our sun has been derived from motion in space, we are hardly warranted in denying that the light and heat possessed by the stars were derived from another source. It is true that the motion of the stars in relation to one another, or in relation to our system (and this is the only motion known to us), is but trifling in comparison to what we even witness in our solar system. But this is what we ought, *a priori*, to expect; for if their light and heat were derived from motion in space, like that of our sun, then, like the sun, they must have lost their motion. In fact, *they are suns, and visible because they have lost their motion.* Had not the masses of which these suns were composed lost their motion they would have been non-luminous, and of course totally invisible to us. In short, we only see in stellar space those bodies which, by coming into collision, have lost their motion, for it is the lost motion which renders them luminous and visible.‡

The formation of a sun by collision is an event that would not be likely to escape observation if it occurred within the

---

* *Philosophical Magazine*, May, 1868; February, 1867. "Climate and Time," chap. xx. *See* also "Transactions of the Geological Society of Glasgow," vol. iii.

† "Student's Manual of Geology," p. 91 (second edition).

‡ *See* "Climate and Time," p. 331.

* *Philosophical Magazine* for May, 1868.

† *Philosophical Magazine*, May, 1868; "Climate and Time," chapter xxi.

‡ When the foregoing theory of the origin of the sun's heat was advanced, in 1868, I was not aware that a paper on the "Physical Constitution of the Sun and Stars" had been read before the Royal Society by Mr. G. Johnstone Stoney, in which he suggested that the heat possessed by the stars may have been derived from collisions with one another. "If two stars" he says, "should be brought by their proper motion very close one of three things would happen: Either they would pass quite clear of one another, in which case they would recede to the same immense distance asunder from which they had come: or they would become so entangled with one another as to emerge from the frightful conflagration which would ensue, as one star; or, thirdly, they would brush against one another, but not to the extent of preventing the stars from getting clear again." In the latter case he considers a double star is formed. Mr. Stoney's paper, though read in 1867, was not published till 1869.

Mr. Herbert Spencer, in his "First Principles" (pp. 532-535), has also directed attention to the fact that the stars distributed through space must tend, under the influence of gravity, to concentrate and become locally aggregated. Separate aggregations will be drawn toward one another, and ultimately coalesce. The result will be that the heat evolved by such collisions taking place under the enormous velocities acquired by gravity must have the effect of dissipating the matter of which they are composed into the gaseous state.

Both Mr. Stoney and Mr. Spencer consider the motion of the cosmic masses to be due wholly to gravity, but, as we have seen, gravity alone cannot account for the enormous amount of energy originally possessed by the sun.

limits of visibility in space. But such an event must be of very rare occurrence, or the number of stars visible would be far greater than it is. The number of stars registered down to the seventh magnitude, inclusive, is—according to Herschel—somewhere between 12,000 and 15,000, and this is all that can possibly be seen by the naked eye. Now, if we suppose each of them to shine like our sun for (say) 100,000,000 years, then one formed in every 7,000 or 8,000 years would maintain the present number undiminished. But this is the number included in both hemispheres, so that the occurrence of an event of such unparalleled splendor and magnificence as the formation of a star—or rather nebula, for this would be the form first assumed—is what can only be expected to be seen on our hemisphere once in about 15,000 years.

The absence of any historical record of such an event having ever occurred can therefore be no evidence whatever against the theory.

NOTE ON SIR WILLIAM THOMSON'S ARGUMENTS FOR THE AGE OF THE EARTH.

SIR WILLIAM THOMSON has endeavored to prove the recent age of the earth by three well-known arguments of a purely physical nature: The first is based on the age of the sun's heat; the second, on tidal retardation of the earth's rotation; and the third, on the secular cooling of the earth.

Argument from the Age of the Sun's Heat.—It will be obvious that, if what has already been advanced in regard to the origin of the sun's heat be correct, it will follow that the argument for the recent age of the earth, based upon the assumption that the sun could have derived its store of heat only from the condensation of its mass, must be wholly abandoned, and that, in so far as this argument is concerned, there is no known limit to the amount of heat which the sun may have possessed, or to the time during which it may have illuminated the earth.

Argument from Tidal Retardation.—It is well known that, owing to tidal retardation, the rate of the earth's rotation is slowly diminishing; and it is therefore evident that, if we go back for many millions of years, we reach a period when the earth must have been rotating much faster than now. Sir William's argument is,* that had the earth solidified several hundred millions of years ago, the flattening at the poles, and the bulging at the equator, would have been much greater than we find them to be. Therefore, because the earth is so little flattened, it must have been rotating, when it became solid, at very nearly the same rate as at present. And as the rate of rotation is becoming slower and slower, it cannot be so many millions of years back since solidification took place. A few years ago I ventured to point out† what appeared to be a very obvious objection to this argument, viz., that the influence of subaerial denudation in altering the form of the earth had been entirely overlooked. It has been proved, as we have seen, that the rocky surface of our globe is being lowered, on an average, by subaerial denudation, at the rate of about one foot in 6,000 years. It follows, as a consequence, from the loss of centrifugal force resulting from the retardation of the earth's rotation occasioned by the friction of the tidal wave, that the sea-level must be slowly sinking at the equator, and rising at the poles. This, of course, tends to protect the polar regions and expose equatorial regions to subaerial denudation. Now, it is perfectly obvious that, unless the sea level at the equator has, in consequence of tidal retardation, been sinking during past ages at a greater rate than one foot in 6,000 years, it is physically impossible the form of our globe could have been very much different from what it is at present, whatever may have been its form when it consolidated, because subaerial denudation would have lowered the equator as rapidly as the sea sank. But in equatorial regions the rate of denudation is, no doubt, much greater than one foot in 6,000 years, because there the rainfall is greater than in the temperate regions. It has been shown that the rate at which a country is being lowered by subaerial denudation is mainly determined not so much by the character of its rocks as by the sediment-carrying power of its river-systems. Consequently, other things being equal, the greater the rainfall the greater will be the rate of denudation.

We know that the basin of the Ganges, for example, is being lowered by denudation at the rate of about one foot in 2,300 years; and this is probably not very far from the average rate at which the equatorial regions are being denuded. It is therefore evident that sub-aerial denudation is lowering the equator as rapidly as the sea-level is sinking from loss of rotation, and that consequently we cannot infer from the present form of our globe what was its form when it solidified. In as far as tidal retardation can show to the contrary, its form, when solidification took place, may have been as oblate as that of the planet Jupiter. There is another circumstance which must be taken into account. The lowering of the equator, by the transference of materials from the equator to the higher latitudes, must tend to increase the rate of rotation, or, more properly, it must tend to lessen the rate of tidal retardation.

The argument may be shown to be inconclusive from another consideration. The question as to whether the earth's axis of rotation could ever have changed to such an extent as to have affected the climate of the poles, is at present exciting a good deal of attention. The subject has recently been investigated with great care by Prof. Houghton,‡ Mr. George Darwin,§ the Rev. J. F. Twisden,‖ and others, and the general result arrived at may be expressed in the words of Mr. G. Darwin: "If the earth be quite rigid, no redistribution of matter in new continents could ever have caused the deviation of the pole from its present position to exceed the limit of about 3 deg."

Mr. Darwin has shown that, in order to produce a displacement of the pole to the extent of only 1 deg. 46', an area equal to one twentieth of the entire surface of the globe would have to be elevated to the height of two miles. The entire continent of Europe elevated two miles would not deflect the pole much over half a degree. Assuming the mean elevation of the continents of Europe and Asia to be 1,000 feet, Prof. Houghton calculates that their removal would displace the pole only 199·4 miles.

It may now be admitted as settled that if the earth be perfectly rigid the climate of our globe could never possibly have been affected by any change in the axis of rotation. But it is maintained that if the earth can yield as a whole, so as to adapt its form to a new axis of rotation, the effects may be cumulative, and that a displacement of the pole as much as 10 deg. or 15 deg. is possible.

But then if the earth be able to adapt its form to a *change in the axis of rotation*, there is no reason why it may not be able to adapt its form to a *change in the rate of rotation*, and if so, the flattening at the poles and the bulging at the equator would diminish as the rate of rotation diminished, even supposing there were no denudation going on.*

Argument from the Secular Cooling of the Earth.—The earth, like the sun, is a body in the process of cooling, and it is evident that if we go back sufficiently far we shall reach a period when it was in a molten condition. Calculating by means of Fourier's mathematical theory of the conductivity of heat, Sir William Thomson has endeavored to determine how many years must have elapsed since solidification of the earth's crust may have taken place. This argument is undoubtedly the most reliable of the three. Nevertheless, the data on the subject are yet very imperfect, so that no definite and trustworthy result can be arrived at by this means as to the actual age of the earth. In fact, this is obvious from the very wide limits assigned by him within which solidification probably took place.

"We must," quoting Sir William's own words on the subject, "allow very wide limits on such an estimate as I have attempted to make, but I think we may, with much probability, say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have—nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of 'emergence' of the 'consistentur status' was probably within these dates."†—*Quarterly Journal of Science.*

A BIOGRAPHICAL SKETCH OF AN INFANT.

By CHARLES DARWIN.

M. TAINE'S very interesting account of the mental development of an infant, translated in the last number of *Mind* (page 252), has led me to look over a diary which I kept thirty-seven years ago with respect to one of my own infants. I had excellent opportunities for close observation, and wrote down at once whatever was observed. My chief object was expression, and my notes were used in my book on this subject; but, as I attended to some other points, my observations may possibly possess some little interest in comparison with those by M. Taine, and with others which hereafter no doubt will be made. I feel sure, from what I have seen with my own infants, that the period of development of the several faculties will be found to differ considerably in different infants.

During the first seven days various reflex actions—namely, sneezing, hiccoughing, yawning, stretching, and, of course, sucking and screaming, were well performed by my infant. On the seven day I touched the naked side of his foot with a bit of paper, and he jerked it away, curling at the same time his toes, like a much older child when tickled. The perfection of these reflex movements shows that the extreme imperfection of the voluntary ones is not due to the state of the muscles or of the co-ordinating centres, but to that of the seat of the will. At this time, though so early, it seemed clear to me that a warm soft hand applied to his face excited a wish to suck. This must be considered as a reflex or an instinctive action, for it is impossible to believe that experience and association with the touch of his mother's breast could so soon have come into play. During the first fortnight he often started on hearing any sudden sound, and blinked his eyes. The same fact was observed with some of my other infants within the first fortnight. Once, when he was sixty-six days old, I happened to sneeze, and he started violently, frowned, looked frightened, and cried rather badly: for an hour afterward he was in a state which would be called nervous in an older person, for every slight noise made him start. A few days before this same date he first started at an object suddenly seen; but for a long time afterward sounds made him start and wink his eyes much more frequently than did sight; thus, when 114 days old, I shook a pasteboard box with comfits in it near his face, and he started, while the same box when empty, or any other object shaken as near or much nearer to his face, produced no effect. We may infer from these several facts that the winking of the eyes, which manifestly serves to protect them, had not been acquired through experience. Although so sensitive to sound in a general way, he was not able, even when 124 days old, easily to recognize whence a sound proceeded, so as to direct his eyes to the source.

With respect to his vision, his eyes were fixed on a candle as early as the ninth day, and up to the forty-fifth day nothing else seemed thus to fix them; but on the forty-ninth day his attention was attracted by a bright-colored tassel, as was shown by his eyes becoming fixed and the movements of his arms ceasing. It was surprising how slowly he acquired the power of following with his eyes an object if swinging at all rapidly; for he could not do this well when seven and a half months old. At the age of thirty-two days he perceived his mother's bosom when three or four inches from it, as was shown by the protrusion of his lips and his eyes becoming fixed; but I much doubt whether this had any connection with vision; he certainly had not touched the bosom. Whether he was guided through smell, or the sensation of warmth, or through association with the position in which he was held, I do not at all know.

The movements of his limbs and body were for a long vague and purposeless, and usually performed in a jerking manner; but there was one exception to this rule, namely, that from a very early period, certainly long before he was forty days old, he could move his hands to his own mouth. When seventy-seven days old, he took the sucking-bottle (with which he was partly fed) in his right hand, whether he was held on the left or right arm of his nurse, and he would not take it in his left hand until a week later, although I tried to make him do so; so that the right hand was a week in advance of the left. Yet this infant afterward proved to be left-handed, the tendency being no doubt inherited—his grandfather, mother, and a brother having been or being left-handed. When between eighty and ninety days old, he drew all sorts of objects into his mouth, and in two or three weeks' time could do this with some skill; but he often first touched his nose with the object and then dragged it down into his mouth. After grasping my finger and drawing it to his mouth, his own hand prevented him from sucking it; but on the 114th day, after acting in this manner, he slipped his own hand down so that he could get the end of my finger into his mouth. This action was repeated several times, and evidently was not a chance but a rational one. The intentional movements of the hands and arms were thus much in advance of those of the body and legs; though the purposeless movements of the latter were from a very early period usually alternate, as in the act of walking. When four months old, he often looked intently at his own hands and other objects close to him, and in doing so the eyes were turned much inward, so that he often squinted frightfully. In a fortnight after this time (*i.e.*, 132 days old) I observed that if an object was brought as near to his face as his own hands were, he tried to seize it, but often failed; and he did not try to do so in regard to more distant objects. I think there can be little doubt that the convergence of his eyes gave him the clew and excited him to move his arms. Although this infant thus began to use his hands at an early period, he showed no special aptitude in this respect, for, when he was two years and four months old, he held pencils, pens, and other objects, far less neatly and efficiently than did his sister, who was then only fourteen months old, and who showed great inherent aptitude in handling anything.

Anger.—It was difficult to decide at how early an age anger was felt; on his eighth day he frowned and wrinkled his skin round his eyes before a crying-fit, but this may have been due to pain or distress, and not to anger. When about ten weeks old, he was given some rather cold milk and he kept a frown on his forehead all the time that he was sucking, so that he looked like a grown-up person made cross from being compelled to do something which he did not like. When nearly four months old, and perhaps much earlier, there could be no doubt, from the manner in which the blood gushed into his whole face and scalp, that he easily got into a violent passion. A small cause sufficed; thus, when a little over seven months old, he screamed with rage because a lemon slipped away and he could not seize it with his hands. When eleven months old, if a wrong plaything was given him, he would push it away and beat it; I presume that the beating was an instinctive sign of anger, like the snapping of the jaws by a young crocodile just out of the egg, and not that he imagined that he could hurt the plaything. When two years and three months old, he became a great adept at throwing books or sticks, etc., at any one who offended him; and so it was with some of my other sons. On the other hand, I could never see a trace of such aptitude in my infant daughters; and this makes me think that a tendency to throw objects is inherited by boys.

Fear.—This feeling probably is one of the earliest experienced by infants, as shown by their starting at any sudden sound when only a few weeks old, followed by crying. Before the present one was four and a half months old, I had been accustomed to make close to him many strange and loud noises, which were all taken as excellent jokes, but at this period I one day made a loud, snoring noise which I had never done before; he instantly looked grave and then burst out crying. Two or three days afterward, I made through forgetfulness the same noise, with the same result. About the same time (viz., on the 137th day) I approached with my back toward him and then stood motionless: he looked very grave and much surprised, and would soon have cried, had I not turned round; then his face instantly relaxed into a smile. It is well known how intensely older children suffer from vague and undefined fears, as from the dark, or in passing an obscure corner in a large hall, etc. I may give as an instance that I took one child in question, when two and a quarter years old, to the Zoological Gardens, and he enjoyed looking at all the animals which were like those that he knew, such as deer, antelopes, etc., and all the birds, even the ostriches, but was much alarmed at the various larger animals in cages. He often said afterward that he wished to go again, but not to see "beasts in houses;" and we could in no manner account for this fear. May we not suspect that the vague but very real fears of children, which are quite independent of experience, are the inherited effects of real dangers and abject superstitions during ancient savage times? It is quite conformable with what we know of the transmission of formerly well-developed characters, that they should appear at an early period of life, and afterward disappear.

Pleasurable Sensations.—It may be presumed that infants feel pleasure while sucking, and the expression of their swimming eyes seems to show that this is the case. This infant smiled when forty-five days, a second when forty-six days old; and these were true smiles, indicative of pleasure, for their eyes brightened and eyelids tightly closed. The smiles arose chiefly when looking at their mother, and were therefore probably of mental origin; but this infant often smiled then, and for some time afterward, from some inward pleasurably feeling, for nothing was happening which could have in any way excited or amused him. When 110 days old, he was exceedingly amused by a pinafore being thrown over his face and then suddenly withdrawn; and so he was when I suddenly uncovered my own face and approached his. He then uttered a little noise which was an incipient laugh. Here surprise was the chief cause of the amusement, as is the case to a large extent with the wit of grown-up persons. I believe that for three or four weeks before the time when he was amused by a face being suddenly uncovered he received a little pinch on his nose and cheeks as a good joke. I was at first surprised at humor being appreciated by an infant only a little above three months old, but we should remember how very early puppies and kittens begin to play. When four months old, he showed in an unmistakable manner that he liked to hear the pianoforte played; so that here apparently was the earliest sign of an æsthetic feeling, unless the attraction of bright colors, which was exhibited much earlier, may be so considered.

Affection.—This probably arose very early in life, if we may judge by his smiling at those who had charge of him when under two months old; though I had no distinct evidence of his distinguishing and recognizing any one, until he was nearly four months old. When nearly five months old, he plainly showed his wish to go to his nurse. But he did not spontaneously exhibit affection by overt acts until a little above a year old, namely, by kissing several times his nurse who had been absent for a short time. With respect to the allied feeling of sympathy, this was clearly shown at six months and eleven days by his melancholy face, with the corners of his mouth well depressed, when his nurse pretended to cry. Jealousy was plainly exhibited when I fondled a large doll, and when I weighed his infant sister, he being then fifteen and a half months old. Seeing how

* "Transactions of the Geological Society of Glasgow," vol. iii., p. 1.
† *Nature*, August 21, 1872; "Climate and Time," p. 336.
‡ "Proceedings of the Royal Society," vol. xxvi., p. 51.
§ "Proceedings of the Royal Society," vol. xxv., p. 328.
‖ Paper read before the Geological Society, February 21, 1877.

* A displacement of the pole of less than 15° or 20° would be of very little service in accounting for the warm climate of Greenland during the Miocene and other periods. But a displacement to that extent, even supposing that we admit the earth to be yielding, demands a condition of things which few geologists would be willing to grant. When it becomes generally recognised what an enormous extent the temperature of the arctic regions is dependent upon ocean-currents, the difficulties in understanding how those regions have once enjoyed a temperate climate will disappear. Were the ice removed from Greenland that region would at present enjoy a warm summer suitable for plant and animal life. It is the presence of ice rather than a positive deficiency of heat that makes Greenland so cold and barren (*see* "Climate and Time," chapter iv.). An increase in the quantity of heat conveyed by ocean-currents, merely sufficient to prevent the accumulation of ice, would completely transform the climate of the arctic lands. And such an increase would take place during an inter-glacial period when the eccentricity of the earth's orbit was at a high value and the winter solstice in perihelion.
† "Transactions of the Royal Society of Edinburgh," vol. xxiii., p. 161.

strong a feeling jealousy is in dogs, it would probably be exhibited by infants at an earlier age than that just specified, if they were tried in a fitting manner.

Association of Ideas, Reason, etc.—The first action which exhibited, as far as I observed, a kind of practical reasoning, has already been noticed, namely, the slipping his hand down my finger so as to get the end of it into his mouth; and this happened on the 114th day. When four and a half months old, he repeatedly smiled at my image and his own in a mirror, and no doubt mistook them for real objects; but he showed sense in being evidently surprised at my voice coming from behind him. Like all infants he much enjoyed thus looking at himself, and in less than two months perfectly understood that it was an image; for if I made quite silently any odd grimace, he would suddenly turn to look at me. He was, however, puzzled at the age of seven months, when being out-of-doors he saw me on the inside of a large plate-glass window, and seemed in doubt whether or not it was an image. Another of my infants, a little girl, when exactly a year old, was not nearly so acute, and seemed quite perplexed at the image of a person in a mirror approaching her from behind. The higher apes which I tried with a small looking-glass behaved differently; they placed their hands behind the glass, and in doing so showed their sense, but, far from taking pleasure in looking at themselves, they got angry and would look no more.

When five months old, associated ideas arising independently of any instruction became fixed in his mind: thus as soon as his hat and cloak were put on, he was very cross if he was not immediately taken out-of-doors. When exactly seven months old, he made the greatest step of associating his nurse with her name, so that if I called it out he would look round for her. Another infant used to amuse himself by shaking his head laterally: we praised and imitated him, saying "Shake your head;" and when he was seven months old he would sometimes do so on being told without any other guide. During the next four months the former infant associated many things and actions with words: thus when asked for a kiss he would protrude his lips and keep still—would shake his head and say in a scolding voice "Ah!" to the coal-box or a little spilt water, etc., which he had been taught to consider as dirty. I may add that when a few days under nine months old he associated his own name with his image in the looking glass, and when called by name would turn toward the glass even when at some distance from it. When a few days over nine months, he learned spontaneously that a hand or other object causing a shadow to fall on the wall in front of him was to be looked for behind. While under a year old, it was sufficient to repeat two or three times at intervals any short sentence to fix firmly in his mind some associated idea. In the infant described by M. Taine (pp. 254–256) the age at which ideas readily became associated seems to have been considerably later, unless indeed the earlier cases were overlooked. The facility with which associated ideas due to instruction and others spontaneously arising were acquired, seems to me by far the most strongly marked of all the distinctions between the mind of an infant and that of the cleverest full-grown dog that I have ever known. What a contrast does the mind of an infant present to that of the pike, described by Professor Möbius,* who during three whole months dashed and stunned himself against a glass partition which separated him from some minnows; and when, after at last learning that he could not attack them with impunity, he was placed in the aquarium with these same minnows, then in a persistent and senseless manner he would not attack them!

Curiosity, as M. Taine remarks, is displayed at an early age by infants, and is highly important in the development of their minds; but I made no special observation on this head. Imitation likewise comes into play. When our infant was only four months old I thought that he tried to imitate sounds; but I may have deceived myself, for I was not thoroughly convinced that he did so until he was ten months old. At the age of eleven and a half months he could readily imitate all sorts of actions, such as shaking his head and saying "Ah!" to any dirty object, or by carefully and slowly putting his finger in the middle of the palm of his other hand, to the childish rhyme of "Pat it and pat it, and mark it with T." It was amusing to behold his pleased expression after successfully performing any such accomplishment.

I do not know whether it is worth mentioning, as showing something about the strength of memory in a young child, that this one, when three years and twenty-three days old, on being shown an engraving of his grandfather, whom he had not seen for exactly six months, instantly recognized him, and mentioned a whole string of events which had occurred while visiting him, and which certainly had never been mentioned in the interval.

Moral Sense.—The first sign of moral sense was noticed at the age of nearly thirteen months. I said, "Doddy" (his nickname) "won't give poor papa a kiss—naughty Doddy!" These words, without doubt, made him feel slightly uncomfortable; and at last, when I had returned to my chair, he protruded his lips as a sign that he was ready to kiss me; and he then shook his hand in an angry manner until I came and received his kiss. Nearly the same little scene recurred in a few days, and the reconciliation seemed to give him so much satisfaction that several times afterward he pretended to be angry and slapped me, and then insisted on giving me a kiss. So that here we have a touch of the dramatic art, which is so strongly pronounced in most young children. About this time it became easy to work on his feelings, and make him do whatever was wanted. When two years and three months old he gave his last bit of gingerbread to his little sister, and then cried out with high self-approbation, "O kind Doddy, kind Doddy!" Two months later he became extremely sensitive to ridicule, and was so suspicious that he often thought people who were laughing and talking together were laughing at him. A little later (two years and seven months and a half old) I met him coming out of the dining-room with his eyes unnaturally bright, and an odd, unnatural or affected manner, so that I went into the room to see who was there, and found that he had been taking pounded sugar, which he had been told not to do. As he had never been in any way punished, his odd manner certainly was not due to fear, and I suppose it was pleasurable excitement struggling with conscience. A fortnight afterward I met him coming out of the same room, and he was eying his pinafore, which he had carefully rolled up; and again his manner was so odd that I determined to see what was within his pinafore, notwithstanding that he said there was nothing, and repeatedly commanded me to "go away," and found it stained with pickle-juice; so that here was carefully-planned deceit. As this child was educated solely by working on his good feelings, he soon became as truthful, open, and tender as any one could desire.

Unconsciousness, Shyness.—No one can have attended to very young children without being struck at the unabashed manner in which they fixedly stare without blinking their eyes at a new face; an old person can look in this manner only at an animal or inanimate object. This, I believe, is the result of young children not thinking in the least about themselves, and therefore not being in the least shy, though they are sometimes afraid of strangers. I saw the first symptom of shyness in my child when nearly two years and three months old: this was shown towards myself, after an absence of ten days from home, chiefly by his eyes being kept slightly averted from mine; but he soon came and sat on my knee and kissed me, and all trace of shyness disappeared.

Means of Communication.—The noise of crying, or rather of squalling, as no tears are shed for a long time, is, of course, uttered in an instinctive manner, but serves to show that there is suffering. After a time the sound differs as to the cause, such as hunger or pain. This was noticed when this infant was eleven weeks old, and I believe at an earlier age in another infant. Moreover, he appeared soon to learn to begin crying voluntarily, or to wrinkle his face in the manner proper to the occasion, so as to show that he wanted something. When forty-six days old, he first made little noises without any meaning to please himself, and these soon became varied. An incipient laugh was observed on the 113th day, but much earlier in another infant. At this date I thought, as already remarked, that he began to try to imitate sounds, as he certainly did at a considerably later period. When five and a half months old he uttered an inarticulate sound. "Da!" but without any meaning attached to it. When a little over a year old he used gestures to explain his wishes. To give a simple instance, he picked up a bit of paper, and, giving it to me, pointed to the fire, as he had often seen and liked to see paper burned. At exactly the age of a year he made the great step of inventing a word for food—namely, *mum;* but what led him to it I did not discover. And now, instead of beginning to cry when he was hungry, he used this word in a demonstrative manner or as a verb, implying "Give me food." This word, therefore, corresponds with *ham* as used by M. Taine's infant at the later age of fourteen months. But he also used *mum* as a substantive of wider signification; thus he called sugar *shu-mum*, and a little later, after he had learned the word black, he called licorice *black-shu-mum*—black-sugar-food.

I was particularly struck with the fact that when asking for food by the word *mum* he gave to it (I will copy the words written down at the time) "a most strongly-marked interrogatory sound at the end." He also gave to "Ah!" which he chiefly used at first when recognizing any person or his own image in a mirror, an exclamatory sound, such as we employ when surprised. I remark in my notes that the use of these intonations seemed to have arisen instinctively, and I regret that more observations were not made on this subject. I record, however, in my notes that at a later period, when between eighteen and twenty-one months old, he modulated his voice in refusing peremptorily to do anything by a defiant whine, so as to express "That I won't;" and again his humph of assent expressed "Yes, to be sure." M. Taine also insists strongly on the high-expressive tones of the sounds made by his infant before she had learned to speak. The interrogatory sound which my child gave to the word *mum* when asking for food is especially curious; for if any one will use a single word or a short sentence in this manner, he will find that the musical pitch of his voice rises considerably at the close. I did not then see that this fact bears on the view which I have elsewhere maintained, that before man used articulate language, he uttered notes in a true musical scale, as does the anthropoid ape Hylobates.

Finally, the wants of an infant are at first made intelligible by instinctive cries, which after a time are modified in part unconsciously, and in part, as I believe, voluntarily, as a means of communication—by the unconscious expression of the features—by gestures and in a marked manner by different intonations—lastly by words of a general nature invented by himself, then of a more precise nature imitated from those which he hears; and these latter are acquired at a wonderfully quick rate. An infant understands to a certain extent, and as I believe at a very early period, the meaning or feelings of those who tend him, by the expression of their features. There can hardly be a doubt about this with respect to smiling; and it seems to me that the infant whose biography I have here given understood a compassionate expression at a little over five months old. When six months and eleven days old he certainly showed sympathy with his nurse on her pretending to cry. When pleased after performing some new accomplishment, being then almost a year old, he evidently studied the expression of those around him. It was probably due to differences of expression, and not merely of the form of the features, that certain faces clearly pleased him much more than others, even at so early an age as a little over six months. Before he was a year old, he understood intonations and gestures, as well as several words and short sentences. He understood one word, namely, his nurse's name, exactly five months before he invented his first word *mum;* and this is what might have been expected, as we know that the lower animals easily learn to understand spoken words.—*Mind.*

* "Die Bewegungen der Thiere," etc., 1873, p. 11.

THE FUR-SEAL.

THE fur-seal, or sea-bear, like the sea-lion, is one of the eared seals; and so closely resembles it in external form and in all its movements and habits, that it is hardly to be distinguished from it by any but a skilled observer. The most important difference between them, having regard to their commercial value, is in the outer covering of their skins. The sea-lion, having a hairy coat, is useful chiefly to the natives of the islands and coasts which it visits or inhabits; whilst the fur-seal, haunting the same localities, has a skin, which, from its rich, soft undergrowth of fine fur beneath the longer grizzly hair, is a valuable article of trade with every civilized nation in the world. The breeding habits of the two animals are so nearly identical also, that it will be unnecessary for me to repeat details which will be found in my preceding remarks on the sea lion; I will, therefore, recount only that portion of the history of the fur-seal which belongs to itself apart from its congener, and will take the northern species, *Otaria ursina*, as the type for description.

The male attains to his full size when six years old. He then measures from 7 ft. to 8ft. in length and about the same in girth; and when in full flesh weighs from 500lb. to 700lb. An adult female is about 4ft. long, and 3ft. round the body, and weighs from 80lb. to 100lb. She breeds when three years old, and is full-grown in her fourth year. This difference in the size of those of the two sexes is very remarkable; but as the male is in the habit of taking unto himself from ten to fifteen wives, and as many more as he can get, it may be well for him that he is endued with sufficiently superior strength to enable him to keep them all in order.

On the islands in Behring Sea, which are the principal breeding-places of the northern fur-seal, the snow melts from the shores during the month of April in each year; and about the 1st of May, a few old male seals, the veterans of the herd, make their appearance in the water near the islands, and, after two or three days of careful reconnoissance, shyly and warily venture on shore. Climbing the slopes or rocky ledges, they lie listening with head erect in attitudes of the keenest watchfulness and attention. If they find all quiet, and are undisturbed, these scouts of the main body quietly depart, and a few days afterwards males of all ages begin to arrive—slowly and in small parties at first, but towards the end of the month in countless thousands; and in a very few days acres of ground are covered by the animals as closely as they can be packed together. The old and full-grown "bulls" land at once, and secure positions for the expected occupants of their harems. These and no others are allowed to take places on the "rookeries." The young males under six years of age are compelled to remain in the water during the day, but make their way to the uplands, or to the outskirts of the colony to rest at night, by neutral passages set apart at convenient distances, by which they are permitted to go to and return from the water unmolested.

About the 12th of June the first of the "cow" seals come up from the sea, and then commence the fierce contests between the males for the possession of them which I have described as taking place among the sea-lions.

The cows appear to be led to the land by an instinctive knowledge of the approach of the end of the period of their gestation, for their "pups" are born within a day or two, sometimes within two or three hours, after their arrival. They seldom, if ever, have more than one at a time, and within a few days after its birth are *enceinte* again. The pups when born, and for about three months afterwards, are darker in color than their parents, being almost black on the back, and ashy grey below. By the 15th of September they are five or six times their original weight, and begin to shed their dark hair and take on their second coat. The moulting lasts till the middle of October. This second coat, or seagoing jacket, consists of a dense, light under-fur, greyish in some and light fawn in others, the staple length of which is about half an inch, and of a coarse over-hair two-thirds of an inch long, and of a dark chinchilla grey, blending into greyish white on the belly and chest. In the meantime the young ones have learned to swim, and to feel at home in the water. Mr. Elliott* so graphically describes a pup taking its first bath—an incident which has been recently witnessed in the case of the young sea-lion at Brighton—that I cannot do better than quote his words. After alluding to the statement of some writers that the cub has to be forced into the water by its parents, he says: "The pup blunders awkwardly into the water the first time, and gets out again as quickly as it can, but so far as showing any fear or dislike of this, its natural element, as soon as rested it is ready for a new trial. It makes slow and clumsy progress at first, floundering about when only a head in depth in the most awkward manner with its fore flippers, and not using the hinder ones. In a minute at most the youngster is so weary, that he crawls out upon the rocks or beach, and immediately takes a nap, repeating the lesson when he awakes and is rested. During this period of self-tuition he seems thoroughly to enjoy the exercise. He soon becomes familiar with the water, and delights in it, swims, twists, turns, and dives, and when tired, draws himself up on the beach, and, shaking himself like a dog, goes to sleep again, or has a frolic with his juvenile companions."

At the end of July, the pups being able to take care of themselves, the rookery is to a certain extent broken up. The old bulls go to the sea, and are not seen again till the following season. The standing places they have for two months so desperately conquered and defended are left in possession of the cows and pups; and the young males or "bachelors," as they are termed, delighted apparently in the departure of their tyrannical seniors, either make free use of their shore leave, or plunge and leap and tumble and frolic in the water as the champion swimmers of the community. The cows, having "got their children off their hands," lead a life of placid enjoyment, basking and lazily fanning and scratching themselves with their flippers. The pups gambol and play together by the dozen, and roll over and over, and pretend to bite each other like so many kittens, and all classes seem to be happy and sociable, and to revel in their peaceful life, after the turmoil, and violence, and jealousy, and strife to which they had been accustomed whilst the surly old pashas held control over their seraglios. About the 18th of August—it is astonishing how certainly and punctually the various events of the life-history of the seals may be looked for, as regularly as the coming of the swallows in England—the bachelors and the elders of the tribe, all but the pups, become "stagey," as it is called, *i.e.*, begin to shed their coats. The coarse hair and the fine fur are alike got rid of, and are replaced by a fresh growth of each. The process of their full renewal, from the commencement of the dropping out or falling off of the old coat, occupies about six weeks, and then, by the beginning of October, their winter clothing is perfect. At the end of October they commence to leave the rookery, and by the middle of November the greater number have departed, and they do not revisit the land—at any rate, the same land—until their usual season, the month of May and June in the following year. Some uncertainty exists concerning the direction in which they migrate. Mr. Elliott believes, and probably correctly, that they spread themselves over the North Pacific, following schools of fish, or frequent shoals and banks where an abundance of cod, etc., (their natural food) is found. They sleep soundly on the surface of the water, and in this position are often captured by the natives all the way along the northwest coast, from the Columbia River to Behring Sea. During this season they probably are themselves preyed upon by sharks, "killers" (grampuses), etc., in accordance with the really beneficent, though apparently cruel, law of nature by which the balance of animal life is maintained, and the undue increase of particular species prevented.—HENRY LEE *in Land and Water.*

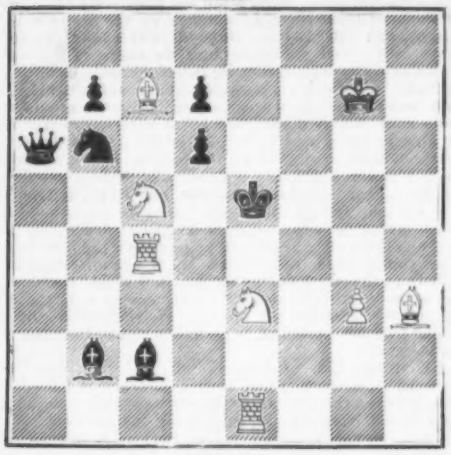
GRIEF OF AN ABANDONED DOG.—A Newfoundland dog was recently left in Louisville by a gentleman who moved to Philadelphia. The dog, homeless and friendless, lay down at the door of his deserted home and gave vent to piteous lamentations. The neighbors tried to comfort him with food and kind words, but in vain. A week after his abandonment he sought shelter in a cellar, and lay insensible to both kindness and cruelty. It was believed that he had become insane. To end his sorrow, he was shot.

Scientific American Chess Record.

[All contributions intended for this department, may be addressed to Samuel Loyd, Elizabeth, N. J.]

Problem No. 5.—By Harry Boardman.

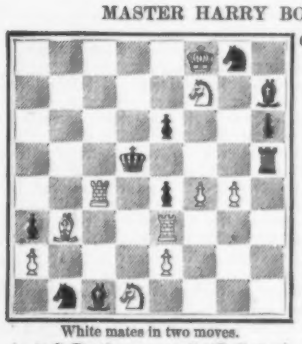
Black.



White.

White to play and mate in three moves.

MASTER HARRY BOARDMAN.



White mates in two moves.

BOTH the regular problem as well as letter fantasia of the present number are selections from the compositions of Master Harry Boardman, of Melrose, Mass., a young problemist who has carried off some of the highest honors in our recent problem tournaments. A glance at his youthful features as depicted in our portrait gallery of this week shows him to be a mere lad just entering upon his teens, and surprisingly juvenile for one who has already composed over a hundred scientific and elaborate chess problems which have gained him a worldwide reputation for skill and ingenuity.

The three mover with which we head this column, carried off the first prize against a host of celebrities in the Detroit *Free Press* tournament of last year.

The two mover (on third column) received the prize for second best two move problem of the Centennial Problem Tournament.

The Initial diagram received second prize in the recent *American Chess Journal* letter tournament.

CHESS ACROSTICS.

The *Amateur World* offered two prizes for the best Acrostic on the word chess. There was quite a large competition, and the prizes have just been awarded as follows:

FIRST PRIZE.

Chess is such a noble game,
How it does the soul inflame!
Ever pleasing, ever new,
Surely Chess has not its due;
Sad to say, 'tis known to few!
W. Harris (Wollaston).

SECOND PRIZE.

Can strife be banished from the homes of men,
Hence, by a charm to its own dinsome den?
Each lover of *The Game* will answer, yes!
Such is thy pow'r to sway us, gentle Chess,
Serene thy realm, Caissa, our Princess.
Neil Kennedy (Glasgow).

This second one recalls to mind one I perpetrated in German several years ago, in reply to a suggestion that a certain legal squabble might be adjusted over a chess board. I now translate it with a free hand to my mother tongue as follows:

Could we at Chess all disputes try
How quick we'd thrash the smaller fry!
Each Rook, or Knight, or Pawn and two
Should be compell'd his words to chew.
Steinitz would be my lawyer too!

THIRD AMERICAN CHESS CONGRESS.

The third meeting of the American Chess Congress was held in Chicago, July, 1874, eight representative players taking part in the contest.

The only terms of the contest required to be mentioned are that each player was to play two games with every other player. Drawn games to score as half a game to each player, fifteen moves an hour to be made on both sides.

The player scoring the greatest number of games to receive *one-half* of the fund subscribed for prizes.

The second best score to receive *one-third*, and the third best score to receive *one-sixth*, being the balance of the fund.

The following was the finale score at the end of the tournament.

Capt. McKenzie	New York	8½	games	1st prize,	$225.
H. L. Hosmer	Chicago	8	"	2d "	$150.
Max Judd	St. Louis	7	"	3d "	$75.
Frederick Bock	Chicago	4	"		
J. A. Congdon	Philadelphia	1½	"		
Frederick Perrin	Brooklyn	1	"		

The score being so close between McKenzie and Hosmer, we select for our specimen game one played between these gentlemen, which may be looked upon as the decisive game of the contest, as upon this victory depended the question as to who should receive first and second prizes. It will be of interest for the student to note that this game, which is also a Ruy Lopez, differs materially in the opening moves from the games we have already published; a conflict of opinions will be found—but as we give our authorities we are not responsible for the same—even at the critical point of this game where 19. R X P and administers what the critics seem to consider a mortal wound. The position was shown to me and I replied by 15. B to B 5, and won the game. I have not had leisure to examine it yet, but am surprised that it was overlooked.

Harry Boardman

(RUY LOPEZ.)

McKenzie.	Hosmer.
1. P to K 4	1. P to K 4
2. Kt to K B 3	2. Kt to Q B 3
3. B to Q Kt 5	3. P to Q R 3
4. B to R 4	4. Kt to K B 3
5. Castles	5. Kt X K P
6. P to Q 4	6. P to Q Kt 4
7. B to Kt 3	7. P to Q 4
8. P X K P	8. B to K 3 (*a*) (B)
9. B to K 3	9. B to K 2
10. Q Kt to Q 2 (C)	10. Kt to Q B 4
11. Q to K 2	11. P to Q 5 (*d*) (E)
12. B to K B 4	12. Kt X K B
13. R P X Kt	13. Kt to Q Kt 5 (*f*)
14. P to Q B 3	14. Kt to Q B 7 (*g*) (H)
15. R X R P (*i*)	15. P to Q 6 (J)
16. R X R	16. P X Q
17. R X Q ch	17. K X R
18. R to Q B	18. B to K B 4
19. P to K Kt 4	19 B to Q 6
20. K to Kt 2	20. P to K Kt 4
21. B to K 3	21. P to Q B 4
22. Kt to K sq	22. Kt X Kt ch
23. R X Kt	23. P to K R 4 (*k*) (L)
24. P to K R 3	24. P X P
25. P X P	25. P to K B 3
26. Kt to K B 3 (*m*)	26. P X P (N)
27. Kt X K P	27. B to K 5 ch
28. P to K B 3	28. B to Q 4
29. R X P	29. R to K B sq (*o*) (P)
30. R to Q 2	30. Resigns.

The series of Notes marked with small letters are by W. N. Potter, from the London *Chess Magazine*, those in caps are by Zuckertort, from the *Westminster Papers*.

(*a*) Kt to K 2, or B to Kt 2 may be played advantageously here. (B) Anderson prefers 8, Kt to K 2, following up by B to Kt 2.

(C) 10, P to Q B 3 was preferable.

(*d*) The advance of this pawn is tempting enough, but its wisdom is not so certain as its boldness. (E) A premature advance.

(*f*) A move totally at variance with all sound principles.

(*g*) Wild in the extreme. Merely scratching the cuticle of the position would have disclosed the utter absurdity of such a line of play. (H) Black overlooks the proper continuation:

	14. P to Q 6
15. Q to K 4	15. B to Q 4
16. Q to B 5	16. P to Kt 3
17. Q to R 3 (best)	17. Kt to B 7
18. Q R to Q sq	18. P to K R 4, and black

has the better game.

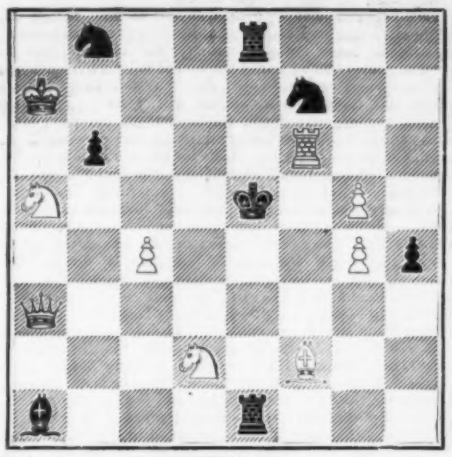
(*i*) A mortal wound.

(J) The best move under the circumstances, as R X R would lose another pawn by 16. Q X P ch.

(*k*) Of what use this desperate attempt to attack white where he is strongest? If any chance remained it could only be by moving the king, and bringing the rook round to the queen's side. (L) A good move, which gives black a chance of an attack against adverse king.

Problem No. 6.—By Harry Boardman.

Black.



White.

White to play and mate in two moves.

(*m*) I doubt very much the wisdom of this move.

26. P X P	26. B X B

27. Kt to B 3 followed by Kt to Kt sq was the best course.

(N) Black throws away a fine chance; 26. B to K 5 at once would be very troublesome for white. The following variation will show that he cannot capture either pawn:—

	26. B to K 5
27. R X P	27. P to B 4
28. B to B sq	28. B to B 3
29. K to Kt 3	29. P to B 5
30. K to Kt 2	30. R to R 5
31. R to Q 2 ch	31. K to K sq
32. R to Q 3	32. P to Q B 5
33. P X P	33. P X P
34. R to Q 4	34. R X P ch
35. K to B sq	35. B X Kt
36. R X Q B P	36. R to R 5
37. K to K sq.	37. R to R 8 ch
38. K to Q 2	38. R to B 8 and black

will win easily.

Or

27. R X P	27. B to Q 3
28. P to B 7 (28)	28. K to K 2
29. R X P	29. K X P
30. B to B 4	30. B X Kt ch
31. K X B	31. B X B and black

ought to win.

(28) B X Kt P	28. R to R 7
29. K to Kt sq	29. B X Kt
30. P to B 7 dis ch	30. K to B 2 and black

forces the mate.

(*o*) Of course a blunder, but care cannot be expected where there is no hope. Our examination of various games of Mr. Hosmer's makes us aware that the want of forethought and soundness of his play in the present game is not to be taken as characteristic of his usual style.

(P) A blunder, which loses a piece; but black's game was past redemption. If

	29. B X P
30. Kt to B 6 ch	30. K to Q 3
31. Kt X B	31. K X Kt

32. B to Q 4 dis ch etc.

THE CENTENNIAL PROBLEM BOOK.

As many of our readers are doubtless aware, it was the original intention of the projectors of the Centennial Problem Tournament to issue in book form a complete collection of the competing problems, as well as the umpire's award, and such other matter as might be supposed to possess interest for the chess public.

The matter has at last been taken in hand, and we may reasonably look for a work that will reflect credit upon American Chess.

It will contain portraits of all of the winners, as well as those of other distinguished competitors, and will also contain a valuable contribution from the skillful pen of Mr. George Carpenter, in the form of a complete record of all of the problem tournaments ever held in this country, as well as the winning problems.

To add to the popularity of the work a cheap edition will be issued for one dollar.

The question of authority in the matter is set at rest by the following complimentary letter which has been tendered to Mr. Cook and Dr. Moore:

"The thanks of the Editorial Fraternity are hereby tendered to our distinguished umpire, Mr. Eugene B. Cook, and worthy secretary, Dr. C. C. Moore, for their thorough and satisfactory labors in our Grand Centennial Problem Tournament.

"Trusting that you will look upon it as an unqualified endorsement of the happy results of your labors, and not a further encroachment upon your good nature, we express the hope that one or both of you, gentlemen, will take charge of the preparation and publication of the proposed Centennial Problem Book.

"Most respectfully,

J. N. Babson, of the *Boston Globe*.
J. B. McKim, Cleveland *Sunday Voice*.
T. P. Bull, Detroit *Free Press*.
M. J. Hazeltine, *N. Y. Clipper*.
J. Elson, *Philadelphia Record*.
B. S. Foster, *St. Louis Democrat*.
M. Lenan, Newark *Sunday Call*.
Capt. McKenzie, *Turf, Field and Farm*.
W. Hallock, *Am. Chess Journal*.
Samuel Loyd, *Scientific American*.
D. M. Neill, *Danbury News*.
A. P. Barnes, *Sporting Life, New Yorker*.
R. L. C. White, *Lebanon Herald*."